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THE PRESENT STATUS AND FUTURE ORIENTATION OF RESEARCH ON THE FAMILY*

LEONARD S. COTTRELL, JR.
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THE SUGGESTION of the program committee that we use this opportunity to appraise the status of research on the family and to consider the direction and orientation of future research in this field is a timely one. In some quarters there is an impression that family research is now suffering from neglect on the part of sociologists; that much of the research which is being done is disjunctive, lacks orientation to important problems; and is not "adding up" to significant new knowledge; that there is little evidence of imagination in the formulation of creative hypotheses. While one may question whether or not the picture is as discouraging as that, it must be admitted that even a cursory review of research published in say the last three years and the reports of the research in progress does give some basis for these impressions. One does at least get the feeling that while a substantial number of sociologists and social psychologists are actively conducting research on various aspects of the family, there is little or no concerted attack on a carefully selected series of problems which are agreed upon as fundamental. For the most part, research is being conducted by scattered and isolated investigators who are frequently

working on relatively trivial problems. This is not to say that we are entirely lacking in productive effort by high caliber investigators. But in my opinion the doubts and misgivings are sufficiently well grounded to justify the investment of considerable effort in orienting research on the family so that our energies will be more effectively mobilized for concerted and persistent attack on what we can agree are significant problems.

PRESENT STATE OF KNOWLEDGE

We are very fortunate that in our present undertaking of review and appraisal we can draw heavily upon an excellent article recently published by Ernest W. Burgess¹ in which he discusses our present state of knowledge and points to some important gaps which require new research. The availability of this statement by Burgess enables us to devote a minimum of space in this paper to reviewing the present state of our knowledge and makes it possible for us to devote most of our discussion to the problem of a productive orientation for future research.

The following is a brief summary of Burgess' main findings:

1. Our most adequate and accurate knowledge of the family is with respect to

* Paper read before the annual meeting of the American Sociological Society, New York City, New York, December 28-30, 1947.

¹ The Family and Sociological Research, *Social Forces*, V. XXVI, Oct., 1947, 1-6.

the trends which can be indicated statistically; size of the family, rates of marriage and divorce, urbanization, economic base, loss of certain historic institutional functions, etc. There are certain trends for which statistical indices are not readily available but which are indicated by other types of evidence clearly enough to warrant the confident assertion that the changes are taking place. These trends are in the changing concepts of the family, e.g., from authority to equalitarian control; from institutional relations to companionship relations; from familism to individualism; from integration based on roles defined in the mores to that which is based on much more individualized patterns of relationship. Some of the implications of these trends for family stability, adjustment in the family, and personality development are already evident; but an adequate understanding of their full impact waits upon more research.

In connection with his discussion of trends, Burgess points to the need for more data on the family which could be obtained by the inclusion of more detail on marriage licenses and divorce petitions and in census enumeration forms. He also points to the fruitful possibilities of accumulating attitude and opinion material on marriage and the family through public opinion research operations.

2. There is a large body of knowledge accumulated for the most part by psychiatrists and child psychologists, on the dynamics of inter-personal relations and personality developments in the family. Sociologists have not contributed nearly as much as might be expected to theory and methods in this research. A review of work in this area shows clearly that it would benefit greatly by the introduction of a better integration of the concepts of culture with personality theory and a more adequate theory of social interaction. It is in these directions that sociologists with special interest in social psychology could contribute an extremely valuable frame of reference and methodology.

In this connection it should be pointed out that we need to extend the work already

done in the description of structures and dynamics of family relations in the different segments of our own society. We need comparative descriptive studies of family patterns in the different urban, rural, class and ethnic settings, together with analyses of the personality structures which emerge from these different family patterns. So far there is no work of this type which compares with the study of the Negro family in the United States by E. Franklin Frazier.²

3. We have made a reasonably good beginning in research on family crises which seem to threaten the family stability, the factors affecting the ability of the family to resolve the crisis situation, and the processes by which adjustments are made. In these studies the concept of the role has proved to be of great importance.

4. During the past fifteen years the greatest advance in our knowledge of the family has been in the field of mate selection and marital adjustment. Sociologists have made very significant contributions to both theory and method, particularly with respect to the prediction of adjustment in marriage. Substantial progress has been made in defining and measuring adjustment in courtship and marriage; in demonstrating significant statistical relationships between a variety of pre-marital background characteristics and adjustment in courtship and marriage relations; and in demonstrating that a fair degree of accuracy is possible in the classification of couples according to probabilities for good adjustment.

In addition to the statistical analyses and prediction, important advances have been made in the analysis of personality patterns which persons bring to courtship and marriage and in demonstrating how these patterns operate in determining the nature of the marriage relations. Studies of the dynamics of marital interaction and the factors affecting marital adjustment continue to be a focus of great interest and activity. However, workers in this area will be the first to admit that we are just beginning to scratch the surface.

²Frazier, E. F., *The Negro Family in the United States*. Chicago: University of Chicago Press, 1939.

5. The sociology and social psychology of sex has hardly been touched. The result of extensive studies conducted by Alfred C. Kinsey³ as well as clinical findings of more intensive analyses has contributed much to our knowledge in this area, but there is a definite need to place behavior described in studies such as that by Kinsey and in clinical findings in their proper social interactional contexts if we are adequately to understand sexual behavior.

6. No summary would be complete without reference to research in what might be called homemaking or family management. Studies of standards of living, economics of the home, child care, family nutrition and health, housing, and similar matters have resulted in an imposing body of facts which are of great practical importance. However much of this work is of a scattered and piecemeal variety and requires a much more adequate systematization than now obtains.

7. The sense of need for understanding the problem of the family has not only served to motivate research, but has also given rise to efforts to apply knowledge in education for family life and family counselling. There appears now to be a growing opinion that research is needed to evaluate the effects of family life education and of family counselling. There are obviously great difficulties in such evaluative studies, but they certainly offer opportunities for research which would have great importance not only from a practical point of view but theoretically and methodologically as well.

Neither an outline such as the foregoing or an article such as the one by Burgess could possibly do justice to the task of summarizing the present state of our knowledge about the family. They serve merely to point out the main foci of recent research interest and to give some indication of the nature of the knowledge which is resulting from such research. In my opinion there is a real need now, even with our incomplete knowledge, for a detailed systematic work on the subject, "What We Know About the Family." I am reasonably confident that we

ourselves would be as surprised at how much we know as at how much we do not know.

ORIENTATION FOR FURTHER RESEARCH

With this brief review as a background we can now turn to the question of what should be the directions and foci of future research on the family. The answers to this question will, of course, be as varied as the different ways investigators perceive the family and structure its problems. It is my opinion, however, that research could be more highly productive in the next decade, say, if we could achieve enough of a consensus in our definition of the situation to enable us to make a concerted attack on what could be agreed upon as the more important problems, and in so doing use a common frame of reference for our analyses and interpretations. If this were done by a substantial proportion of investigators, the results of the efforts would be more likely to "add up to something" than if each of us selects our problems and hypotheses more or less haphazardly. I am not by any means suggesting that we attempt to limit any of the freedoms, but simply that we are at a point where some intensive efforts at developing some common perspectives and pooling our efforts to achieve some common objectives will greatly facilitate more rapid progress toward significant and useful results.

The research proposals which I shall suggest here are offered in the hope that they will provoke other opinions. In discussing contrasting views, we may arrive at some common orientations which may lead to collaborative effort among ourselves and investigators in other relevant scientific disciplines.

To consider the orientations I wish to suggest, we need to view the family as a functional mechanism. Various investigators have pointed out that through changes in the character of our social structure the family has progressively lost many of the social functions for which it formerly was primarily responsible. From the evidence it seems that the greatest losses in function have been in areas of activity classified as

³ Kinsey, A. C., *Sexual Behavior in The Human Male*. Philadelphia: W. B. Saunders, 1948.

economic, educational, recreational, protective, and religious.

Let us assume this to be the case. (Although it should be noted parenthetically that some very important economic, educational, etc., functions do remain even in highly urbanized families and, further, that research on these particular functional aspects has probably been too much neglected.) Notwithstanding the loss of many of its social functions there remain several very important ones for which the family is and will probably continue to be the most important social machinery. Most of us would agree that probably the most important of these remaining functions are: reproduction; the satisfaction of the intimate affectional response needs; and socialization and personality patterning. I strongly suspect that great advances will be made in family research if a considerable number of investigators would make these functions the foci of concerted attack and would coordinate their efforts in developing a series of basic hypotheses with respect to each and direct this research toward testing such hypotheses. In the interest of stimulating some discussion of the desirability of the kind of focus and coordination suggested, I offer the following observations on each of the functional areas suggested above.

1. *Reproduction.* I shall not elaborate on the possibilities in this functional area because I wish to use the available time on the other two areas which, I think are in greater need of structuring for systematic research. This neglect does not indicate any tendency on my part to give a low priority to problems centering around the reproductive functions of the family. Those interested in my suggestions in this field may refer to an article on social psychological aspects of differential fertility which appeared in 1937.⁴ Some of the research proposals made there are still relevant. Furthermore, the intensive work on the social and psychological factors affecting fertility being conducted by Kiser and

Whelpton⁵ will undoubtedly produce significant results and serve as a model and stimulus for a continued and broadened collaborative research in this area.

2. *The Satisfaction of the Affectional Response Needs.* Those who have studied human relations in the family have reported a wide variety of patterns in which the affectional responses have been cast. There are some people who seem to require an affectional relation in which they are always on the receiving end of pampering behavior. Others can love only in a situation where the object of their affections is a weak, dependent person. Still others establish what we vaguely refer to as a mature affectional relation. There are complicated patterns in which the object of affection is also the object of hostile destructive behavior. There are people who literally do not know how to be loved and some of them never learn. There are some who never learn how to give love to another person.

We not only find a wide variety of affectional patterns in a given population but we also know that in the course of development of the individual he passes through a series of different patterns of affectional interaction. Now the Freudian psychoanalysts have, of course, developed an imposing theory and body of clinical observation on the patterning of the libidinal drives, and there can be no question of the tremendous amount of insight and understanding they have developed. There is certainly no need to deny the value of their theoretical formulations or their empirical findings. But it seems to me we are now at a point where we need a fresh look at the problem in the interest of a more comprehensive formulation of theory in terms of interpersonal dynamics.

There is definite need to take theory and findings in psychoanalysis, psychosomatics, interpersonal dynamics, and cultural anthropology and build a more comprehensive theory of affectional patterning. This is no

⁴ Cottrell, L. S. Jr., "Research in Causes of Variations in Fertility: Social Psychological Aspects," *Amer. Sociological Review*, II, 1937, 678-685.

⁵ See Kiser, C. V. and Whelpton, P. K. "Progress Report on the Study of Social and Psychological Factors Affecting Fertility," *Amer. Sociological Review*, XII, April, 1947, 175-186.

easy task and it certainly calls for intensive collaboration among a number of investigators who are trained in the different fields and who also (and this is very important) are the kinds of personalities who can share and communicate and integrate new points of view into their own thinking and orientation.

With a more comprehensive theoretical formulation we should then seek to:

- (a) Describe the variety of patterns of affectional relations;
- (b) Trace the processes through which they develop and change;
- (c) Determine which of these patterns make for the most stable and affectionally satisfying marriage and parent-child relations; and
- (d) Describe the kind of treatment and training of children and adolescents in the family, as well as in the schools and character building agencies which promote and hinder the development of these preferred patterns.

3. *Socialization and Personality Patterning.* Students of the family are constantly pointing out that the family is the matrix of social experience in which the basic socialization of the organism takes place and in which the most persistent and pervasive personality patterns are formed. If this be the case then it would appear highly desirable that we focus a substantial amount of intensive collaborative effort in theoretical formulation and in planning and conducting research to test relevant hypotheses regarding the factors and processes in socialization.

Socialization is a term which is rather loosely used in the literature. In some contexts it has a normative valuational connotation; in others it is a term of general reference to learning of the habits and attitudes characteristic of a social group. It seems to me that whatever else the term may imply it refers to a process whereby social interactive systems become incorporated into the response system of the individual. This means that a basic process involved in social relations is what we can call the empathic responses. That is to say those reactions whereby the individual takes the

role of the others. It is my opinion that in all social integration, all social interaction, communication and understanding, and even in thought itself, the empathic responses are of central importance.

It is therefore surprising that so little attention has been given to research on this fundamental factor in social relations. And it is for this reason that I would urge that a great deal of research in the family be oriented to an exhaustive study of empathic reactions and abilities. We need to know how to measure empathic ability. We need to know the range and distribution of the ability under various life conditions. We need to analyze the kinds of interactive experiences in infancy, childhood and adolescence which promote and hinder the development of this ability. Can the ability be modified by training? Are persons characterized by high empathic ability in a given situation better adjusted, more insightful, and better able to handle conflicts than those who rate low in this ability? These and many other questions point to serious gaps in our knowledge of a critical factor in socialization. Family research workers should be in a particularly good position to contribute greatly to the development of theory and methods for studying empathic responses and their roots in the context of family interaction.

Empathic ability and its development, manifestations and implications is not the only aspect of personality development in the family which should concern students in this field. (I have, of course, already referred to the patterning of affectional responses as another important focus of research effort.) There are many other important personality characteristics which are developed primarily in the family context about which investigators have already accumulated a great deal of knowledge. However, much of this work has been done with a focus and orientation appropriate to the clinician's preoccupation with the individual and his special characteristics and problems of personal adjustment. It is probably attributable to our basic cultural orientation to the values of individualism

that we have not raised serious research questions about the implications of individual personality patterning in the family for our social system. What the family does to the individual person is important not only because it largely determines his individual patterns of social relations and adjustment but also because it determines the extent to which his attitudes and behavior are consistent with and supportive of the social system of which he is a part. In view of this latter point I should like to propose another orientation for family research. In brief this research would be directed to the question of what kind of personality characteristics and skills are required for adequate functioning in democratic relations and what kind of family interaction develops those traits and skills. In outlining this orientation I shall merely quote from a memorandum I recently prepared on this subject.

1. Every society is confronted by the task of molding its constituent personalities in such a way that their personality organizations are supportive of that society and can function in the social system with a reasonable amount of consistency and adjustment. Some societies accomplish this more or less informally and unsystematically; others are more deliberate, systematic, and self-conscious in accomplishing this end.

2. It is assumed by many students of the family, with considerable justification, that the family is a major institutional device for the transmission of the basic value systems and personality characteristics required by a given society for its continuation. This is not to say, of course, that other institutional components of a society, such as school, church, et cetera, do not also contribute heavily to the patterning process, but for the moment our attention is focused on the family.

3. It is assumed here for purposes of this discussion that the weight of opinion in the United States is in favor of maintaining and extending a system of values and correlative patterns of human relations which may be described as democratic. This assumption will, of course, have to be carefully examined at the appropriate time.

4. Paragraphs 1, 2, and 3 lead to the following questions:

- a. In certain critical areas of human relations in our society how should people behave, what attitudes should they have, and what skills in dealing with situations should they possess, if they are to react in a manner consistent with democratic values and supportive of a democratic system?
 - b. What basic personality characteristics must they have if they are to react in such a manner with the minimum of anxiety and frustration, or in other words if they are to be well adapted to function in democratic human relations?
 - c. What kinds of treatment and training of children and adolescents in the family are conducive to the production of the basic personality characteristics and social skills called for in *a* and *b*?
 - d. Similarly what kinds of treatment and training encourage the development of those characteristics and skills antithetical to democratic human relations?
5. In the opinion of the writer, the adoption of an explicit orientation suggested in the foregoing assumptions and questions would give much more pointed relevance to family research than it now has, both from a practical and a theoretical standpoint. Such an orientation should not only be productive of new and highly relevant hypotheses regarding personality formation in the family and the relation between the structure in the family and that of the larger society, but would also serve as a means of organizing and focusing existing hypotheses that are relevant to this field. Studies of change in family patterns should take on new significance when cast in this context. Comparative studies of the families in different cultures would have a much more explicit theoretical as well as practical relevance and significance than such studies now have. Child development studies obviously could be given more point and direction in some such frame of reference as is suggested above. An important by-product of such an effort would be further clarification in behavioral terms of our conception of what are democratic human relations and a focusing of attention on the possible contribution of social science to achieving these relations.
6. Notes and observations:
- a. Research with the proposed orientation could be done with a view to relating personality products of the family to democratic and non-democratic behavior in the wider range of community life generally,

or it could be done with the more limited target of relating personality resultants of family life to democratic and non-democratic behavior in marriage and parent-child relations.

- b. The proposed orientation calls for much more explicit definitions of what constitutes democratic human relations than we now have. Question *a* under Paragraph 4 is an extremely difficult one to answer, but if social scientists are to bring their research to bear explicitly on the problem of a democratic society, this question has to be answered in unambiguous terms.
- c. It is obvious that this orientation could be used for research on other social institutions as well, particularly those institutions which assume responsibility for transmitting the culture and developing the character and skills needed by the society, e.g., the school, the church, character building agencies, recreational agencies, and the like.

CONCLUSION

I have emphasized what appear to me to be important orientations for research on the family. These orientations derive from regarding the family as the major social device for the performance of the function of reproduction; satisfying needs for intimate affectional response; and the function of basic socialization and personality patterning. In offering these proposals I wish to make it quite clear that I am not urging that they supplant present research emphases on family trends, marital adjustment, descriptive study of family group patterns in various segments of our society, comparative studies of the family in various cultures, and other significant lines of attack. Moreover, the suggestions are all within areas of interest which are familiar to students of the family. The merit of the proposals lies chiefly in calling attention to certain critical problems around which a great deal of individual and collaborative research could profitably be organized. Such organized efforts should certainly lead to more significant advances in knowledge than are now likely to come from the random and sometimes trivial investigations which are being reported so frequently now.

It seems to me that the situation is ripe for a major next step in family research. There should be set up under the Social Science Research Council a carefully chosen, well-financed planning group who would devote full time for one or two years to a careful examination of what we know, what the critical problems for research are, the major hypotheses to be tested, and what research methods are available and what new ones are needed in this field. On the basis of their analysis this group should also propose ways and means of facilitating the kinds of collaborative research which are so obviously needed today if we are to make progress which will be commensurate with the need for reliable knowledge. The results of such an analysis and review should provide a basis for developing centers of family research as well as a structuring of the field for the stimulation and guidance of individual investigators. I would anticipate that such a planning operation would develop a new morale in this field and, in part at least, insure that the results of many investigations will begin to "add up."

DISCUSSION

Ernest W. Burgess
University of Chicago

What orientation should we take in our planning of family research?

First of all we can eschew planning and depend on the laissez-faire approach. This would be the opportunistic way of embarking upon projects according to the inclination of the investigator or the chances of securing funds. That policy, if it can be dignified by such a name since it is the absence of policy, has brought us to the stage we are in today of casual fragmentary, scattered and meager research.

A second method is the survey or omnibus approach. This is what I can be fairly charged with in my survey of needed research published in the October, 1947 issue of *Social Forces* and incorporated in condensed form in the first part of Dr. Cottrell's paper. It gives an overall view of the entire field, discloses gaps in our knowledge and may indicate fruitful areas to be further plowed. It is better than the procedure of opportunistic endeavor, but it is not good enough for the best research planning.

Dr. Cottrell proposes that a research program be selective. With that point of view we can perhaps all agree. It is far better to have research guided by some idea of selection than to be without any particular orientation as in the first two methods already considered.

Dr. Cottrell further recommends a specific line of orientation. This is a good principle. The question is not if a given orientation is good, but is it good enough? Is it the best available guide for the selection of research projects?

He proposes as the principle of orientation the study of the functions of the family, particularly those now remaining, namely, those of reproduction, satisfaction of affectional needs, socialization and personality patterning, with especial emphasis upon the preparation of children for democratic behavior in our American society.

Dr. Cottrell has stated cogently and effectively the considerations that should induce research workers to put their money (or more accurately someone else's money) on this horse. But is this the best horse for us to select? Perhaps it has certain handicaps for the research race. Its poor as well as its good points need consideration.

The orientation outlined by Dr. Cottrell throws the emphasis upon parent-child relations as they focus on child development rather than on husband and wife relations that come into the picture only indirectly. A conception of a more integral type is needed: one which would be referent to the unity of family relations would seem to be preferable. The stress on needs, personality patterning, and socialization assumes that these are three separate aspects of family functioning but gives no indication of any central point of unification.

In my reflections on modern family organization I have found this central core of family experience in companionship. This concept of companionship is equally applicable to husband wife relations as to parent child relations. The fulfillment of family needs, personality patterning and socialization take on a new meaning and significance in thinking of them in their relationship to the process of companionship.

The essence of family relations is an intimate companionship of husband and wife, of parents and children, of a type not found elsewhere in social life. In our sociological lingo the essence of family experience is the expression and impression of intimacy, response, and emotional security. Accordingly, I would propose for con-

sideration family research directed to the study of the factors important to the realization of this companionship of husband and wife and parents and children. This may indeed center about the factors mentioned by Dr. Cottrell; namely, reproduction, personality needs, personality patterning, socialization, and democratic relations.

Companionship is, I think, a good principle of orientation, but is it the best principle? May I suggest three others that appear to me to merit consideration and which in fact may turn out to be superior to the ones already suggested?

The first of these is that of adjustment, the extent to which a family succeeds or fails in meeting situations arising directly or indirectly from the conditions of life in a changing society.

The notion of adjustment brings with it emphasis upon adjustability or flexibility. In a static society a high degree of integration was important for family stability. Angell has shown that in a crisis integration is not as important as adaptability for family adjustment and survival. Perhaps empathic ability is an important element in flexibility but it is not the only one. Knowledge and skills are also important.

Closely allied to the concept of adjustment is that of prediction. At present a number of workers are utilizing the prediction approach in family research. This has been in large part due to an interest in a method which would secure findings that were scientifically dependable. The test of prediction is a crucial one: the hypotheses stand up or are demolished.

The test of prediction has another value. It permits and encourages the research worker to use both statistical and case-study methods. Hypotheses based on case studies can be tested by clinical predictions or they can be translated into forms susceptible to statistical treatment on the basis of a large number of cases. The method of prediction tends to place the emphasis upon findings that can be added to our store of verifiable knowledge.

The final orientation I shall suggest is not new but it should not be discounted for that reason. It is the traditional and perhaps still the soundest starting point. It is that of scientific curiosity and the question of what is the most important problem to be attacked which would add to the store of knowledge about human behavior in social relations. This scientific attitude spurns the notion of "Knowledge for What" so eloquently set forth by Robert S. Lynd. Often it confidently affirms that the most

direct way of getting results valuable for practical use is to disregard all notions of practicality. Its adherents affirm the old saw that "the longest way 'round is the shortest way home."

If we take the distinterested view of science where would our studies of family behavior have their central focus?

They would be directed to the definition and the analysis of the family as a group of interacting personalities in interaction with the environmenting community.

They would center upon a selection of problems in relation to the control of the basic variables involved. One of the most important and at the same time the simplest of these is the number of family members. What differences are involved in family interaction by the addition to husband and wife of one, two, three, four, and more children?

The principle of scientific selection would also suggest the choice of those projects where certain of the specific conditions can be controlled, such as those provided by the situation of identical twins reared apart where heredity is the same but the environment may be radically different. Such a project undertaken with adequate provision for biological, cultural, anthropological, psychoanalytic, and sociological insights and techniques would give us basic knowledge on certain of the conditions of the process of family functioning suggested by Dr. Cottrell.

The study of the patterning of personality in family interaction has been proposed by Dr. Cottrell. This is a difficult and complex problem for study. For example, many characteristics of personality have both psychogenic and cultural components. This makes both for the fascination and the frustration of research in personality. Yet both elements need to be identified and isolated at least for purposes of research.

A pertinent illustration is behavior we call dominating and submissive. It is obvious that individuals are superordinate in some activities and subordinate in others according to the roles they are expected to play in specific situations. In marriage in the United States today the situation may become complex and complicated. According to our democratic ideals husbands and wives are equal in family behavior. Psychologically or psychogenically either husband or wife may tend to be dominating or submissive. While our democratic ideology would or should permit either husband or wife to be dominant

there is a powerful tradition, nurtured in the family and in the community, that the husband should be in fact the head of the family. Perhaps the time will come when democracy in our society and the equality of the sexes will be given more than lip service. Until that millennium there will be an abundant opportunity for research on the interplay of these three factors in family life. Such studies, however, require team work between biology, cultural anthropology, psychology, and sociology.

In following any of the orientations suggested for family research it is apparent that we may come out with much the same program of projects of research. The difference may be mainly one of relative emphasis. Yet this difference is to be given most careful consideration.

As suggested by Dr. Cottrell the stage has now been reached in family research where further significant progress calls for two preconditions. These have been stated by Dr. Cottrell but I would like to re-emphasize them:

1. A research center, adequately staffed and supported

2. A multidisciplinary approach.

Research at present on marriage and the family is scattered and sporadic. As a result our research findings are relatively meager, not too reliable, with big gaps in the area uncovered. Almost no one is giving his full time to research in this field. Only in a few cases have even small sums been available for research. Studies have been carried on by individuals working independently on their projects with only the tools of concepts and methods available in their particular discipline.

This is the dark side of the picture. There is a silver lining. Conceptual frames of reference and research methods have been devised which are applicable to research upon the family. The cultural anthropologist has demonstrated the central role of culture and the mores in family organization and personality formation. The psychologist has developed a conceptual system of individual differences, the learning process, and the role of habits and the emotions. The psychiatrist, and particularly the psychoanalyst, has defined and analyzed the process by which emotional attitudes and the psychogenic configurations of personality are formed in the interpersonal relations of family interaction. The social psychologist and sociologist have developed the concepts of role, status, and expectations as determinants of family interaction and personality formation.

Each discipline has developed its own particular kit of research methods and techniques. Cultural anthropology has developed procedures of field work for the study of the family in different cultural settings. This equipment in techniques becomes valuable in studies of the family, not only among other modern peoples but in the sub-cultures of ethnic groups and social classes in our American society. Similarly, psychology is at work on research methods of varying degrees of promise for research in human behavior. These include personality tests, projective techniques, the method of free association and dream analysis. The sociologist has stressed the significance of the personal document and its basic value in revealing processes of behavior in the perspective of the role of the person and his conception of himself.

DISCUSSION

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Dr. Cottrell has suggested three important areas for research and has developed two of them, the family as the source of affectional response needs and as the agency for socialization and personality patterning. These problems are discussed in relation to the "young" family that is in the process of establishing itself and producing and rearing children. Another and almost untouched field for research in these same problems is the aging family. By the aging family I mean the family whose activities and responsibilities are decreasing.

The whole field of research in old age is just beginning to open; imagination is needed to glimpse the possibilities. Research workers in early or middle maturity seem to turn naturally toward the problems of early adulthood and child-rearing. Perhaps this is to be expected, since often they are personally absorbed in these problems; moreover, their own experiences and memories make these areas vivid and realistic. For most young people, research workers as well as others, old age is some dim and dull stage that one will inevitably reach but which offers few enticing experiences. It is not yet widely envisaged as a promising field for research.

The rapid increase in the proportion of the older age groups in the population, however, points to the urgent need for extending research into this area. In 1900, only 4.1% of the population was aged 65 or over and the old age

group was neglected as an insignificant number of people. In 1940, this elderly group equalled 6.8% of the total and by 1980, it is estimated that it will be 14.4% of the population. With the prospect of one person in seven being in the old age group, it is high time that research workers turned their attention to this group.

Much of the needed research pertains to family relationships and is of the type suggested by Dr. Cottrell for the young family. Certain background information must be discovered, however, before successful research in family relationships can be undertaken.

1. More information is needed regarding physical and mental changes that develop with age. This information must come from biologists and psychologists, who fortunately are beginning to develop such research. Most of the physical and mental changes that come with advanced age indicate a decline in function: that is, people have less physical strength, less recuperative power in case of illness, less speed of reaction; mentally, they learn new things more slowly. Since physical and mental changes are limiting in character, they lead to curtailment of activities that were normal for the earlier adult period. These curtailments relate to the family as well as to other fields of activity.

2. Also as background material more information is needed concerning the age-roles developed for old people by society as a whole. These age-roles with their attendant attitudes and activities are established mainly by the younger adult group rather than by old people themselves. The roles are not always closely related to the personal needs of the old and sometimes are in sharp contrast to the roles previously held by these old people as young or middle-aged adults. Consider the abrupt and startling change that comes to a man when he is forced to retire at age 65 and overnight changes his role from that of self-sufficient employed man earning his family's income to that of unemployed and unoccupied man on a modest pension not sufficient for his family's needs. In family and community alike his role and status have been changed, not necessarily in relation to his capacities and needs but because of a social pattern imposed by society. We need to know much more about the roles established for the old as a background for all kinds of specific studies.

The specific problems relating to the family that are here stated are limited to the two fields

elaborated by Dr. Cottrell, the affectional relationship and social interaction as this interaction relates to roles.

1. The affectional relationship of the aging husband and wife merit study. Dr. Cottrell has specifically mentioned the need for describing the variety of patterns of affectional relations and for tracing the processes through which these patterns develop and change. I would urge the extension of this study into the old age period, until death of one spouse terminates the marital relationship. Specifically, what are the ties that bind the husband and wife together when children are no longer a joint responsibility and when sex is a declining function? Are the happy old couple an outgrowth of the happy young couple, the quarrelling oldsters simply the quarrelling young grown old? Or do new mechanisms related to biological and mental changes enter in?

2. What becomes of affectional needs when husband or wife dies? Widows are more typical of the later years than are married women. From age 70 on, more than half of all women are widows; the percentage is less high for men, but increases rapidly with advancing years. Affectional needs seem to continue to the end of life; old men and women who live alone complain of their loneliness and of their desire for companionship with someone of their own generation who understands their point of view and problems. What mechanisms of adjustment are called into play when husband or wife is left alone at a period when children have left the home and the probability of remarriage is negligible? What substitutes do old people find for the broken companionship of a half century? Are they the same compensations that younger widows and widowers find, or are they prevented by failing strength from finding compensations in work or social life? Which substitutes of the old are conducive to their well-being and happiness and which are detrimental?

3. Although at present remarriage of widows and widowers in old age is at a low rate, with increased numbers of old people it seems probable that such remarriages may increase. How can these marriages be reconciled with societal disapproval of marriages of the old? What are the bases for such marriages at a period when sex and children are not dominant factors? What effect does previous marriage have upon the type of husband or wife selected and upon the processes of marital adjustment? Young people, according to one theory at least, tend to select

husbands and wives in the image of the fathers and mothers. Do old people select second husbands and wives in the image of the first and now dead marital partners?

Predictive techniques have been developed to determine the probability of happy marriages for young people. Are these predictive techniques valid for the remarriages of the old?

4. What processes of adjustment take place between the roles of husband and wife when the husband retires or finds himself unemployable because of age? For many years the husband held his status as head of the family primarily because he was the producer of the family income. With unemployability he loses this status except in the minority of families where he has accumulated savings sufficient to provide for himself and his dependents or when he belongs to one of the occupational groups (as teachers, ministers, or public employees) that provides a satisfactory pension. At the same time that his status declines, that of his wife as manager of the home remains unchanged. The situation is further complicated by the fact that often the husband spends his new leisure time in the home. Is his family status now lower than that of his wife, a reversal of earlier roles? Does he try to compensate for his lost status by assuming the dominant role in household management? What adjustments are being worked out by aged couples? Which adjustments give satisfaction to both husband and wife, which lead to conflicts?

5. It seems probable that there may be a revival of the three-generation family, although not as a continuous relationship throughout life. The separation of parents and children at the time of the marriage of the children seems to be well established. But the increasing number of old people and the failure to date of community services to provide adequately for emotional and physical care is causing many persons to look to the adult children of the old people to provide the needed care, which may be financial, physical, or emotional in nature. The alternatives to care by adult children are foster homes where a few old people are cared for in a family other than their own, or institutional care of some sort. With reference to infants, the principle is now accepted that care in a home, preferably that of their parents, provides the best personality development. For old people, the opinion of many social workers is slowly centering on the homes of adult married children as the best place for old people.

Since the close familial relationship of parents and children usually is broken at the time of the children's marriage, inclusion of the aging parents in the family of an adult married child means reestablishing familial interaction after an interval of perhaps twenty years, during which parents and children may have had only infrequent contacts. This situation poses numerous problems for research.

a. What combination of parents and children is most desirable? Ought the widowed mother live with her married son and perhaps revive an unsettled oedipus feeling with resulting antagonism toward her daughter-in-law? Or ought she live with her married daughter and perhaps revive old rivalries? Many such unsolved parental-child tensions and conflicts that were active during the early years of child-rearing and forgotten during the independent adulthood of the children may be revived when adult children must assume the daily care of aged parents. Can these old conflicts be settled or must the old parent and the middle-aged child assume again the roles of the rebellious adolescent and the repressive parent, or of the timid child and the overly protective parent?

b. How can the roles of the aged parents and the roles of the adult children be adjusted to each other? The parents have had the roles of heads of the family for forty or fifty years; when they last lived with their children, the children had subordinate roles with reference to the parents. In the meantime, the children have married and established their own status as heads of families. How can these opposing roles be adjusted without conflict and with satisfaction to both aged parent and adult children? If it is assumed that the adult children should maintain their status as heads of the joint family and the parents relinquish theirs, what compensatory means of status may be provided for the parents? A study of happy and unhappy families in which aged parents have been established in the homes of adult married children after an interval of independence of the two families should yield material valuable not only with reference to the aging family but important in general for the study of adjustment of roles.

c. When there are grandchildren many problems arise regarding the respective roles of grandchildren and grandparents. These problems are especially important when the grandparents have had only casual contacts with the family during the early years when the child's per-

sonality and roles were being formed. For instance, the child who had been the petty tyrant over his parents will not yield gracefully to losing some of the parental attention which now must be directed toward the care of aged grandparents. The situation may be similar to that which arises when a new baby is born into the family, except that in the case of the new baby the care of the parents gradually decreases as the baby grows while in the case of the grandparents the care must increase as the grandparents decline in strength toward the end of their lives. Socialization and personality development of the young now should include satisfactory processes of interaction with grandparents as well as with other members of the family.

DISCUSSION

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I should like to add a few observations concerning certain inter-disciplinary trends in family research. The interpenetration of various disciplines has been a frequent theme of the present meetings and I should like to show how this trend has been reflected in our field. My purpose is not to appraise the following contributions but rather to point out the foci of research and the "explanatory models" emerging in this literature.

Recent anthropological writings on the family are a far cry from the earlier purely descriptive inventories of culture patterns. The anthropologists, sociologists, psychoanalysts have fortunately begun to borrow each other's knowledge and, to a lesser degree, techniques. The studies of primitive families by such writers, for example, as Benedict, Bateson, Kluckhohn, Mead, etc. (as well as their recently emerging and so far largely programmatic writings on contemporary cultures) reveals preoccupation with certain problems. It is noteworthy that these problems are very similar to the proposed foci of research just outlined for us by Professor Cottrell.

Here are some of these problems:

How does a society communicate its cultural heritage to the succeeding generation? More specifically, how are children brought up to be the carriers of its culture? How does, for example, a competitive or a cooperative society ensure its own functioning by conveying proper values and skills to the growing child? Since the family is viewed as the most important of the

transmission agencies, these general problems are being translated into very detailed observations of infant care, child training, and other family patterns.

How does the family determine the patterns of sexual and affectional response in various societies? Is infantile sexuality tolerated or forbidden? Are "parental symbols" diffuse or concentrated as in our own society? What is the authoritarian role of each parent? Who is the disciplinarian? And who the protector? And what is the nature of parental authority? Is it the authority of unquestioned command or the "rational" authority of the expert?

Sex typing of personality is another problem with which this group of students has been concerned. The earlier writings described variations in cultural definitions of male and female personalities and roles. More recently the attention has turned to the developmental point of view, to the acquisition of his sexual role on the part of the growing child. Why do some fail to make the proper psycho-sexual identification? Are mother-daughter, father-son the accepted family alignments or do they run across sex lines? What is the role of the father with regard to children of each sex and age?¹ Benedict's current research project on Contemporary Cultures may be expected to yield contributions to the above problems.

Another class of writings is a product of an actual collaboration of a group of anthropologists with a psychoanalyst. The works of Kardiner, Linton, Du Bois, etc. focus on the effect of family patterns of a given society upon personality structure and, through it, upon some other, so called "secondary" institutions of that society.

The "explanatory model" is more specific. The starting point and the core of the analysis consists of two axes: good infant care versus neglect and rigid early disciplines (sex, elimination, weaning) versus a permissive upbringing. These variables are then related to personality structure in such terms as strong or weak ego, the habitual methods of adaptation, degree of parental "idealization" and other traits. The "basic personality structure" of a people is then shown to affect its folklore, religion and other "secondary" institutions.

Some six or seven primitive cultures and one

contemporary one (Plainville, U.S.A.) were analyzed in terms of the above schema. In every case the psychoanalyst, Kardiner, based his analysis on the data supplied to him by the anthropologists. In one culture (and one only), the Alors, a set of 38 Rorschach tests given to the natives supplied an independent (and, in this case, corroborative) test of the "basic personality structure."

A similar rapprochement of psychology, psychiatry, and social science is observed in the works of certain sociologists on the subcultures of our own society. Davis and Dollard's *Children of Bondage*, Davis and Havighurst's *Father of the Man* have extended our knowledge of class and of Negro-White differences in family types.²

The first two books focus on the process of socialization in the lower and middle class Whites and Negroes. More specifically, the authors attempt to show how differences in "socialization" (now used as an ethical term) are related to class differences in child training. Each class inculcates in its children its own values, through its own system of punishments and, especially, of rewards.

Middle class child training is characterized by the tense insistence of parents on fast and early attainment of middle class values of cleanliness, respect for property, sexual control, control of physical aggression, and a drive for achievement. Middle class parents succeed in part because they can offer present and future rewards for impulse renunciation. In contrast the lower class child is less well "socialized." On the credit side, however, his psychological drives are less frustrated. He is allowed a deeper physical enjoyment of his body and he is spared the anxiety and guilt produced by rigid disciplines.

It is interesting to note that a recent book by Claudia Lewis, *Children of the Cumberland*, builds upon a similar theme of the contrast between rigid and permissive upbringing—a comparison between New York middle class children and a group of children in the Tennessee mountains.

Still another area of family research reflects the interpenetration of several disciplines. Research in Child Psychology on the part of the

¹ The writer wishes to express her gratitude to Dr. Ruth Benedict for an illuminating discussion of these problems in a personal interview.

² In addition to these and to Frazier's *Negro Family in the U.S.*, already mentioned by other speakers, the whole series of books on Negro personality by the American Council on Education throws light on the Negro Family.

psychologists and the clinical approach of the psychiatrists and the Child Guidance personnel are beginning to converge in the study of child behavior and development.

Future Prospects—A general orientation

It is no accident that these interesting developments in family research reflect the interpenetration of various disciplines. An increasing number of studies will require the collaboration of a number of specialists.

To such a collaboration sociologists have an important contribution to make. The anthropologists, with their understandable interest in how the family of one culture differs from that of another, have studied the cultural framework of the family rather than the total interaction and social process within the family. The psychoanalytical discussions often lack adequate recognition of cultural factors and social structure. This, despite noteworthy attempts of a number of psychoanalysts to set forth their

findings within a framework of cultural relativity.

The sociologists, on the other hand, have a great tradition of Cooley, Mead, Park, Burgess, Waller. The conception of social interaction provides, it seems to me, a good general framework for the integration of the special approaches, these dealing with what have been termed genic, psychogenic, sociogenic factors.

Sociologists have also a methodological contribution to make. We have studied more carefully than either the psychiatrists or the anthropologists the frequency distributions of given family attitudes and behavior at least in our own society. We have also gone further in the quests for causes in ascertaining general sequences and associations of factors especially in the field of marriage adjustment. Finally, if we have not yet solved, we have at least faced more seriously the problem of integration of the case study and the quantitative approaches.

COMMUNITY RESEARCH: DEVELOPMENT AND PRESENT CONDITION*

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RESIDENT WIRTH requested this group to review research on the "community," as exemplified in rural and urban sociology and in ecology; and suggested it consider the material in the light of: 1. background; 2. division of labor, and 3. theoretical problems. He asked me to write a summary report, and the other men to evaluate, discuss, and supplement it from the viewpoint of their special fields. The assignment is too large and diffuse to be covered in a detailed manner in the time allotted. This paper, therefore, will establish a few bench marks, set a frame of reference, and sketch in a few salient details.¹ I hope the others will develop a fourth point,

namely, the contributions they believe men working in their special areas have made to sociological knowledge in this field.

HISTORICAL BACKGROUND

American sociologists, in the last half century, have devoted increasing attention to the empirical study of socio-cultural phenomena in localized areas, variously referred to as neighborhoods, towns, cities, communities, regions. No matter what the unit, it was assumed to be an organized structural and functional entity with spatial, temporal, and sociological dimensions. The development of research within this general framework may be divided into three main periods. The first lasted from about 1895 to 1915, the second from 1915 to 1930, and the third from 1930 to the present.

The first period was marked by an emergent interest in the city as "the natural laboratory of social science."² The character

* Paper read before the annual meeting of the American Sociological Society, New York City, December 28-30, 1947.

¹ A systematic effort was made in 1932 to make an inventory of each of the several fields of sociology and to summarize the methods each used to achieve its results. See L. L. Bernard, (editor) *The Fields and Methods of Sociology*, New York, 1934, pp. 52-109, and 286-345, for the papers pertinent to this discussion.

² Quoted from an announcement issued by Columbia University in 1894, when Franklin H.

of this interest was essentially *normative*.³ Consequently, students approached their problems with a set of implicit assumptions as a guide to what they thought community life ought to be; they compared what they saw in the living community with an ideal construct and found that the concrete realities were not congruous with their cherished abstractions. Therefore, since the ideal was assumed to be the normal, the real must be abnormal. Value judgments were implicit in this frame of reference, one of which was the assumption that the role of the investigator was to "expose" the situation he investigated and to "improve" it; that is, to make the real like the ideal community.

In consequence, these normative investigators limited their interests in the real community to its "problem" aspects—poverty, cruelty, alcoholism, crime, prostitution, economic exploitation, and maladjusted immigrants. In the city, their attention was focussed on the area that epitomized these things, the slum; in the country, what they believed to be the decay or malfunctioning of institutions—the church and the family in particular—the migration of young people toward the city, and the disintegration of neighborhoods caught their attention.

This *normative-meliorative* approach dominated the study of community life from roughly 1890 to 1915; it is perhaps best exemplified by the social survey movement.⁴ Although it eventuated in the separation of social work from sociology, the seeds of reform matured more rapidly than those which grew into what I have called analysis, and the roots of this lusty growth are still firmly embedded in soil the sociologist claims as his own. They are evident in the

emphasis sociologists have placed on the study of crime and so-called disorganization and social pathology. Thus, the *normative* approach has left an abiding brand upon sociology in general, and upon community research in particular.

The *normative-meliorative* emphasis gradually gave way to the *analytical* when a few sociologists became interested in looking at particular communities in terms of their history, development, population, and organization in order to determine their extent and nature. The earliest studies with an *analytical* orientation were made by graduate students at the University of Chicago, starting in 1900, but these early studies were strongly influenced by the ideas, attitudes, and goals of the reformers. Nevertheless, they marked a turn in the road in the sociologist's efforts to study objectively the communal collectivity in terms of its relation to the larger society.

Charles J. Bushnell's comparison of the "Stock Yard" and "Hyde Park Communities," with special emphasis on the "Stock Yard Community,"⁵ and John M. Gillette's work on the Calumet area in South Chicago⁶ during 1900 and 1901 were the earliest published detailed studies which were indicative of a trend toward *analysis* rather than reform. Bushnell's study is noteworthy for its use of base maps, tables, and graphs to depict his findings. In passing, it should be noted that he utilized the technique of circles⁷ on maps to show the concentration of charity in certain areas.⁸ Map No. 7⁹ showed that the distribution of Families in Economic Distress, Contributors to the Bureau of Associated Charities, and Average Family Incomes in 1900 formed a gradient

Giddings was appointed to the newly created chair of sociology. See F. L. Tolman, "The Study of Sociology in the Institutions of Learning in the United States," *American Journal of Sociology*, 7: 1907, 797-838.

³ *Ibid.*, see especially, 806.

⁴ See Shelby M. Harrison, *The Social Survey, The Idea Defined and its Development Traced*, New York, 1931; also, Manuel C. Elmer, *Technique of Social Surveys*, 3rd edition, Los Angeles, 1927.

⁵ Charles J. Bushnell, "Some Social Aspects of the Chicago Stock Yards," *American Journal of Sociology*, VII: part I, 145-170; II, 289-330; III, 433-474; IV, 687-702; 1901-1902.

⁶ John M. Gillette, "The Culture Agencies of a Typical Manufacturing Group: South Chicago," *American Journal of Sociology*, VII: part I, 91-121; II, 188-215; 1901-1902.

⁷ Map, *op. cit.*, 308.

⁸ *Ibid.*, 308, for a statement of the relation between family charity and "distress" areas.

⁹ *Ibid.*, opposite, 308.

pattern westward from Lake Michigan between 39th and 55th Streets to Western Avenue. He demonstrated that his variables were related to the different areas he mapped in the same way the ecologists did in the 1920's.¹⁰

The earliest sociological studies of rural communities were made by graduate students at Columbia University, under Giddings' stimulation and direction. Giddings viewed the community as a field laboratory to which the sociologist must turn for inductive study if he was to develop a "Science of Society."¹¹ This influence is apparent in the way his students—James M. Williams, *An American Town* (1906); Warren H. Wilson, *Quaker Hill* (1907); and Newell L. Sims, *A Hoosier Village* (1912)—approached and analysed their data. Each of these authors traced the development of the community studied from its founding to the then present. Briefly, they conceived of the community as a social form integrated around interrelated institutional services, located mainly in a nucleated village which catered to both farmers and village dwellers. These men emphasized the disorganizing effects of communications on the small community, and showed how people and institutions made successive adjustments to the expanding urban world.¹² These studies, although analytical in their orientation, had little immediate influence on sociology outside of reinforcing the conviction of social actionists that American farm, village, town, and city life was changing rapidly and—in their judgment—for the worse.

The second era in the development of community research dates from the publication, in 1915, of Charles J. Galpin's *Social*

Anatomy of an Agricultural Community,¹³ and Robert E. Park's "The City: Suggestions for the Investigation of Human Behavior in the City Environment."¹⁴ Galpin, as is well known, carefully described functional patterns of community relationships within a limited farm-village area of interaction. He demonstrated that each village center in Walworth County, Wisconsin, had a tributary area which it served with specific functions; and that each function—banking, newspapers, trade, high schools, churches, libraries—had a characteristic pattern of its own. He also showed that services performed by the community's institutions were the same for village and farm dwellers, and that both groups were interested equally in the maintenance of community services and activities. In short, town and country people were tied together by functions, interests, and sentiments in a structure that could be mapped, measured quantitatively, and studied objectively. He later called this collectivity the "rurban community."

Galpin's *Social Anatomy of an Agricultural Community* stimulated a line of research among sociologists and economists, in land-grant colleges and agricultural experiment stations, which became exceedingly popular after 1920. These studies were characterized generally by the assemblage of masses of detailed facts, usually statistical in nature, on some particular phase or problem of rural life, usually without explicit theoretical orientation or clear-cut conclusions.¹⁵ An indirect but profound effect of Galpin's study was the way it influenced Park's thinking about the structure of the city. Park remarked to the writer, on several occasions in the late 1930's, that Galpin's *Social Anatomy of an Agricultural Community* brought into focus his own thoughts about the relationships between city growth

¹⁰ Bushnell's Stock Yards Community is regarded currently as a local community and a problem area. See Saul D. Alinsky, "Community Analysis and Organization," *American Journal of Sociology*, 46: 787-808; May, 1941.

¹¹ Tolman, *op. cit.*, 807-808.

¹² For a more adequate summary of these studies see Carl C. Taylor, "Techniques of Community Study and Analyses as Applied to Modern Civilized Societies," in Ralph Linton, (editor) *The Science of Man in the World Crisis*, New York, 1945, 419-424.

¹³ University of Wisconsin Agricultural Experiment Station, Research Bulletin No. 34, Madison, Wisconsin, 1915.

¹⁴ *American Journal of Sociology*, XX: 577-612, Mar. 1915.

¹⁵ Carl C. Taylor, *op. cit.*, 427-431.

and structure, institutional services, neighborhoods, and natural areas.

Park's suggestive paper on *The City* proved to be the point of departure for the many studies of city life undertaken at the University of Chicago immediately after World War I, which resulted in the recognition and development of human ecology as a new approach to the interpretation of the community in the middle 1920's.¹⁶

Empirical research on community life had become a major interest of sociologists by the middle of the third decade of the twentieth century. More than a score of studies of some aspects of rural community life were reported in 1926.¹⁷ By this date, the studies of urban life at the University of Chicago, largely under Park's stimulation, had started to appear in printed form. *The City*¹⁸ had come out in 1925, to be followed by *The Urban Community*¹⁹ in 1926. Subsequent monographs appeared at irregular intervals during the next decade.²⁰

American sociology of the 1920's was

characterized by an assiduous accumulation of multitudinous facts about rural and urban life, the coinage of terms ("rurban"), the enunciation of concepts ("natural area"), the formulation of hypotheses (the Burgess zonal hypothesis of city growth), and the investigation of processes believed to underlie communal growth and structure (competition, invasion—succession, segregation). It should be emphasized that these terms and constructs were derived from sociologists' efforts to generalize about the things they saw in the welter of actual life in local areas. During those years students were so busy "discovering" and building that they did not take time to appraise what they had accomplished.²¹ The rise of human ecology as a new approach to and interpretation of communal life was the most notable development of that period.

The publication of *Middletown*²² in 1929, clearly marked the end of one era and the start of another in the development of community research. *Middletown* focussed attention upon a viewpoint that had been largely neglected by ecologists, as well as by urban and rural sociologists, namely, the interrelations between the daily life of people, their institutional organizations and function, and the social structure of the community. Its influence has become increasingly clear as more and more investigators have framed their work around its organizing concepts—social change, institutional organization and function, and social stratification.

¹⁶ R. D. McKenzie, "The Field and Problems of Demography, Human Geography, and Human Ecology" in L. L. Bernard (editor) *op. cit.*, especially pp. 60-62. For an account of the background of Park's paper on the city see: Robert E. Park, Ernest W. Burgess, and R. D. McKenzie, *The City*, Chicago, 1925, VII-VIII; and Milla A. Alihan, *Social Ecology*, New York, 1938, 2-3.

¹⁷ Charles J. Galpin, H. H. Kolb, Dwight Sander-son, and Carl C. Taylor, "Rural Sociology Research in the United States," *Social Science Research Monograph*, 1927.

¹⁸ Park, Burgess, McKenzie, *op. cit.*

¹⁹ Ernest W. Burgess (ed.) *The Urban Community*, Chicago, 1926.

²⁰ Nels Anderson, *The Hobo*, Chicago, 1923; Frederick M. Thrasher, *The Gang*, Chicago, 1927; Ruth S. Cavan, *Suicide*, Chicago 1927; Ernest R. Mowrer, *Family Disorganization*, Chicago, 1927; Louis Wirth, *The Ghetto*, Chicago, 1928; Harvey W. Zorbaugh, *The Gold Coast and the Slum*, Chicago, 1929; Paul G. Cressey, *The Taxi Dance Hall*, Chicago, 1932; Albert Blumenthal, *Small Town Stuff*, Chicago, 1932; E. Franklin Frazier, *The Negro Family in Chicago*, Chicago, 1932; Walter C. Reckless, *Vice in Chicago*, Chicago, 1933; Bingham Dai, *Opium Addiction in Chicago*, Chicago, 1937; Andrew W. Lind, *An Island Community: Ecological Succession in Hawaii*, Chicago, 1938; and Robert E. L. Faris and H. Warren Dunham, *Mental Disorders in Urban Areas*, Chicago, 1939.

DIVISION OF LABOR AND THEORETICAL PROBLEMS

Community studies published in the last 15 years may be divided into three categories on the basis of their central focus: *ecological*, *structural*, and *typological*. The years since *Middletown* have been marked by a definite decline in the research popularity of the ecological, and an increasing

²¹ This attitude is illustrated at length by the authors represented in L. L. Bernard, *op. cit.*, 52-109 and 286-345.

²² Robert S. and Helen M. Lynd, *Middletown*, New York, 1929.

interest in the structural and typological approaches. The structural, or stratification, approach is in the main a new development. It may be called the prevailing fashion of the third period, just as the investigation of the interrelations between the ecological structure of the community and the pathological aspects of communal life—slums, ghettos, hobohemias, delinquency areas, gangs, ethnic conflict, suicide, and divorce—were very popular in the 1920's.²³

Human Ecology

Ecology has been in an ambivalent position for almost a decade. Alihan's criticism²⁴ fortuitously coincided rather closely with McKenzie's death and Park's retirement from active writing. Although little in the way of theory has been developed in the past decade, the idea persists that there is a legitimate place for human ecology in the social sciences.

As is well known, Alihan's critical analysis, on strictly philosophical grounds, of the thought structure within which empirical ecological studies were made, centered upon ecological theory as propounded by Park, Burgess, and McKenzie. These "classical" ecologists, mainly Park, assumed that the human community was organized around two basic processes, competition and communication, which gave rise to two orders in society—the biotic, rooted in competition, and the moral, rooted in communication.²⁵ With these assumptions as points of departure, Park asserted that the competitive order was the realm of the ecologist and the moral order that of the sociologist. Alihan perceived the dualistic dilemma in which these assumptions placed Park and other orthodox ecological theorists, and moved to the attack with devastating effect on ecological theory.

²³ L. L. Bernard, *op. cit.*, and Carl C. Taylor, *op. cit.*

²⁴ Milla A. Alihan, *op. cit.*

²⁵ R. E. Park, "Human Ecology," *American Journal of Sociology*, XLII: 1-15, Jul. 1936; R. E. Park, "Reflections on Communication and Culture," *American Journal of Sociology*, XLIV: 187-205, Sep. 1938.

Davie before Alihan, and Hiller, Hatt, and Firey since, have subjected selected ecological hypotheses and concepts to specific test at the community level. In 1933, Davie began a systematic study of the physical structure of New Haven to determine the relevancy of Burgess' hypothesis of city growth. He found that it did not apply in this eastern seaboard city of approximately 160,000 people.²⁶ Hiller tested the gradient concept in the areas surrounding 53 small urban centers and found it held true; but the way in which each city influenced the area subordinate to it was dependent upon its size.²⁷ Hatt's application of the *natural area* concept to a section of Seattle indicated that this widely accepted construct did not stand the test of empirical scrutiny at the hands of a trained investigator.²⁸ Although Hatt did not recommend dropping the term from sociological literature, he was careful to point out that it should be used in the specific context of a research problem, and not as a *real* entity.²⁹ Firey has recently published a criticism and test of earlier ecological theories about urban growth in Boston.³⁰ Firey's position runs counter to orthodox views, and his idea of human ecology appears to differ from that of Hawley³¹—a point that may be dis-

²⁶ Maurice R. Davie, "The Pattern of Urban Growth," *Studies in the Science of Society*, New Haven, 1937, 133-161.

²⁷ E. T. Hiller, "Extension of Urban Characteristics into Rural Areas," *Rural Sociology*, 6: 242-257, Sep. 1941.

²⁸ Paul Hatt, "The Concept of Natural Area," *American Sociological Review*, 11: 423-427, Aug. 1946.

²⁹ *Ibid.*, 427.

³⁰ Walter Firey, *Land Use in Central Boston*, Cambridge, Mass. 1947, especially 3-27, for a statement of pre-existing theory, and pp. 323-340 for Firey's own formulation of theory about urban growth. Another recent theory of urban growth is based on the idea of nucleation within the city. For a statement of it see Chauncey D. Harris and Edward L. Ullman, "The Nature of Cities," *Annals of the American Academy of Political and Social Science*, 242: 7-17, Nov. 1945.

³¹ Amos H. Hawley, review of Walter Firey's *Land Use in Central Boston*, *American Sociological Review*, 12: 358-360, June, 1947.

cussed later in this program, since both gentlemen are present.

In spite of the efforts of Quinn,³² Hollingshead,³³ Firey,³⁴ Gettys,³⁵ Hawley,³⁶ and others to re-examine and restate the position of the ecologists, it appears that they have not extricated themselves from the equivocal problem which the classical writers set for them, namely, that the physical structure of the community is a response to the operation of non-cultural determinants operating in a physical and cultural milieu. Hawley recently defined the field of human ecology as the "descriptive study of the adjustment of human populations to the conditions of their respective physical environments." In a clarifying statement of this position he said:

Human ecology, then, may be defined more fully as the study of the development and the form of communal structure as it occurs in varying environmental contexts.

The distinctive feature of the study lies in the conception of the adjustment of man to habitat as a process of community development. This is . . . for human ecology the principle working hypothesis.³⁷

This statement gives ecology a definite focus, but it also heightens the dilemma, for man is an animal living in physical, biotic and cultural environments. This poses the problem: What aspects of the human community as conceived by the ecologist are attributable

to non-cultural and what ones to cultural factors? I wonder how Mr. Hawley, proceeding from his definition, will meet the problem? I also hope that Mr. Firey and Mr. Quinn will respond to this question.

From a strictly theoretical viewpoint one may attribute the adjustments man makes to his habitat, which include the process of community development as well as its structure, to non-cultural factors, as the instinctivists and geographical determinists tend to do, or he may attribute them to purely cultural factors. If one rests his theory on a non-cultural basis, he ignores culture as a conditioning variable and relies upon it merely to illustrate his minor premises. Conversely, if he is a social determinist, he overlooks the non-cultural factors on the one hand, such as climate, terrain, resources, and man as organism, and cultural factors, such as institutions, on the other, and builds his theory almost exclusively on patterned social relations. [Neither position is theoretically justifiable, since man is both an animal and a member of a socio-cultural community, and any particularistic explanation which arbitrarily excludes the natural environment, the individual, society, and culture is untenable.]

To give this phase of the discussion focus, I shall ask the ecologists three questions. First, in the light of a decade of criticism of ecological theory as enunciated by Park, Burgess, and McKenzie, what ecological constructs—principles or concepts—do you consider to be valid today? Second, how would you define or delimit ecology as a field for scientific investigation? If you think that human ecology is a legitimate field of scientific research, and may eventually become a scientific discipline, do you believe it should be viewed as a field of sociology, or as a separate field, in either the biological or the social sciences?

Social Structure

Studies of social structure subsequent to *Middletown* fall into two broad groups: those made by sociologists and those made by social anthropologists, either within the

³² James A. Quinn, "The Nature of Human Ecology—Re-examination and Redefinition," *Social Forces*, 18: 161-168, 1939; "Culture and Ecological Phenomena," *Sociology and Social Research*, XXV: 313-320, Mar. 1941; "The Hypothesis of Median Location," *American Sociological Review*, 8: 148-156, April 1943.

³³ August B. Hollingshead, "A Re-examination of Ecological Theory," *Sociology and Social Research*, 31: 194-204, Jan. 1947.

³⁴ Walter Firey, "Sentiment and Symbolism as Ecological Variables," *American Sociological Review*, 10: 140-204, Jan. 1947.

³⁵ Warner E. Gettys, "Human Ecology and Social Theory," *Social Forces*, 18: 469-476, 1939.

³⁶ Amos H. Hawley, "Ecology and Human Ecology," *Social Forces*, 22: 398-405, May 1944.

³⁷ Amos H. Hawley, *op. cit.*, 404, 405.

Warner group³⁸ or allied with it.³⁹ The Lynds and Warner regarded the social-structure approach as the application of the methods of social anthropology to the study of modern communities.⁴⁰

Both types of "stratificationists" have proceeded on the assumption that inhabitants of a community are aware of differences between themselves which are of such importance to them that their interpersonal relations are organized systematically into horizontally differentiated structures. These distribute the population in social space in such a way that the persons so distributed perform the necessary functions associated with community life; but each stratified segment lives in significantly different ways from the other strata in the system. It is assumed, further, that the essence of community life may be caught by studying the structure of social relations.

The work of the social anthropologists is similar to the sociological product in that it is focussed on the stratification of the community's population into classes, or castes, or both classes and castes. It differs from the sociologist's in its assumptions as to the nature of classes or castes and the effect which stratification has upon personality.

The social anthropologists stratify a community's population, presumably, on the basis of the study of institutional functions, interpersonal relations in associations and cliques, and the attitudes which communal

dwellers voice toward one another. Warner states that his research group "discovered"⁴¹ six classes in Yankee City. But precisely how did they do this? We are not told except by indirection. After the classes were "discovered," the residents of Yankee City were statistically placed in the now familiar scheme, down to the last one-hundredth of one per cent.⁴² I cannot determine from the text either how this was done or by what criteria. At least eight different kinds of data are mentioned: 1. "associational analysis"; "structural participation"; 3. "area lived in"; 4. "type of house"; 5. "kind of education"; 6. "manners"; 7. "other symbols of class"; and 8. "evaluations of members of Yankee City itself";⁴³ but there is no discussion of the ways in which these diverse data were combined, weighted, and pieced together to make an index of class, if, indeed, an index was made. Warner and Lunt apologized for their vagueness when they wrote:

In order to make a complete study it was necessary to locate all of them in one of the six classes, and this we did to the best of our ability on the basis of the entire range of phenomena covered by our data.⁴⁴

The vague procedures of the "Yankee City social anthropologists" stand in sharp contrast to the ones that have been used by sociological students of stratification.⁴⁵ The

³⁸ Warner and Lunt, *op. cit.*, 81-88.

³⁹ *Ibid.*, 88.

⁴⁰ *Ibid.*, 86, 90.

⁴¹ *Ibid.*, 91.

⁴² For examples see: F. Stuart Chapin, *The Measurement of Social Status by the Use of the Social Status Scale*, Minneapolis, Minn., 1933, 3-16; William H. Sewell, *The Construction and Standardization of a Scale for the Measurement of the Socio-Economic Status of Oklahoma Farm Families*, Oklahoma Agricultural Experiment Station, Stillwater, 1940, Technical Bulletin No. 9; George A. Lundberg, "The Measurement of Socio-Economic Status," *American Sociological Review*, 5:29-39, Feb. 1940; Edgar A. Schuler, "Social and Economic Status in a Louisiana Hills Community," *Rural Sociology*, 5: 69-87, Mar. 1940; John Useem, Pierre Tangent, and Ruth Useem, "Stratification in a Prairie Town," *American Sociological Review*, 7: 331-342, June 1942; Harold F. Kauman, *Prestige Classes in a New York Rural Community*, Memoir 260, Cornell University Agricultural Experiment Station, Ithaca,

⁴³ Allison Davis, Burleigh B. Gardner, and Mary R. Gardner, *Deep South*, Chicago, 1941; W. Lloyd Warner and Paul S. Lunt, *The Status System of a Modern Community*, New Haven, 1942; W. Lloyd Warner and Leo Srole, *The Social Systems of American Ethnic Groups*, New Haven, 1945; W. Lloyd Warner and J. O. Low, *The Social System of the Modern Factory*, New Haven, 1947; St. Clair Drake and Horace R. Cayton, *Black Metropolis*, New York, 1945.

⁴⁴ John Dollard, *Class and Caste in a Southern Town*, New Haven, 1937; Hortense Powdermaker, *After Freedom*, New York, 1939; Robert S. and Helen M. Lynd, *Middletown in Transition*, New York, 1937.

⁴⁵ Robert S. and Helen M. Lynd, *op. cit.*, 3-4; W. Lloyd Warner and Paul S. Lunt, *The Social Life of a Modern Community*, New Haven, 1941, 3-5.

sociologists have been more aware of technical problems involved in the stratification of a community and, on the whole, they have used objective criteria and explicit techniques to determine the existence of their classes and to place each person within a given class as determined by their technique. This is a problem of the first magnitude in stratification research, and one which the Warner group in their published writings have largely ignored. However, in passing we must note that Warner has become aware of the problem in recent years, and has developed an *index of status*, but he has not published any materials on it to our knowledge.*

The stratificationists are not clear as to whether "class" or "caste" is a heuristic device useful in making differentiations between people with similar and dissimilar attributes, or an entity in its own right. The terms class and caste have become so familiar in lay and professional usage that they are treated as realities whose characteristics are commonly understood. To illustrate, Treudley has written recently:

Only one individual was interviewed who could be placed definitely in the American lower class, though he had no social contacts even at that level. A few informants either had transferred directly from the upper class in Europe to the same class in the United States or had been so successful here either professionally or artistically as to have achieved an upper class position. More interesting were the informants, again few in number, who were socially isolated from the middle class. Among them were individuals and families who had been in the upper class before their emigration

but who could not, for a variety of reasons, establish a foothold in that stratum over here. They preferred isolation to association with the despised "peasants," who in their view made up the Armenian-American middle class. Again there were a few individuals who would have been perfectly satisfied with a middle class position but who could not make the personality adjustments necessary to fit into either the ethnic or the general community.⁴⁶

I, for one, do not know what an American upper, lower, or middle class is; perhaps Swanson or Kaufman will be able to explain precisely what these terms mean! This tendency to reify is evident in the work of the Warner group, where the six classes are treated as real entities. The crucial question is: Are classes entities or constructs? If they are entities, then any competent person should be able to identify, let us say, an American lower class in terms of objective standardized characteristics in a manner comparable to the differentiation between a duck and a goose. If it is a construct, as I personally believe it is, then it can only be derived by the use of methodological procedures which define a class or caste in terms of specified characteristics. Furthermore, a class or caste so derived should not be reified, certainly not in professional literature.

The stratificationists use "class" or "caste" to explain away the complexities and contradictions in human behavior in a way very reminiscent of the early ecologists. The Lynds made this explicit when they wrote:

... it is after all this division into working class and business class that constitutes the outstanding cleavage in Middletown. The mere fact of being born upon one or the other side of the watershed roughly formed by these two groups is the most significant single cultural factor tending to influence what one does all day long throughout one's life.⁴⁷

New York, 1944, 3-46; Harold Kaufman, "Members of a Rural Community as Judges of Prestige Rank," *Sociometry*, IX: 71-85, Feb. 1946; C. Wright Mills, "The Middle Classes in Middle-Sized Cities," *American Sociological Review*, 11: 520-529, Oct. 1946; August B. Hollingshead, "Selected Characteristics of Classes in a Middle Western Community," *American Sociological Review*, 12: 385-395, Aug. 1947.

*Since this was written Warner has completed his work on the *index of status*. See W. L. Warner, Marjorie L. Merker, and Kenneth S. Eells, *The Measurement of Social Status*, Chicago: Science Research Associates, 1948.

⁴⁶ Mary Bosworth Treudley, "An Ethnic Group's View of the American Middle Class," *American Sociological Review*, 11: 715-724; quotation from 716, Dec. 1946.

⁴⁷ Robert S. and Helen M. Lynd, *Middletown*, 23-24.

Dollard,⁴⁸ Warner,⁴⁹ and Allison Davis⁵⁰ have gone further in this type of interpretation. They have used the class-caste concept to explain personality and mental disorders, without demonstrating the functional relationship between the assumed conditioning factor (class and/or caste), and its asserted processual result (personality structure). Time and space preclude the development of this point, but it could be used as the basis of a critical treatise comparable to the work which Blumer did on *The Polish Peasant*.⁵¹

The Typological Approach

The typologists have studied the community from the viewpoints of its culture, its relations to other communities, its geographic and economic bases, its size, and its population composition, to determine how these factors organize and give meaning to the activities and interpersonal relations of its inhabitants. They are interested in isolating forms of social organization for the purpose of constructing ideal-typical community types. They assume that a given complex of population, culture, and communal organization gives rise to a characteristic way of life, with a correlated complex of meanings and personality types. This idea is rooted deeply in folk beliefs. It is commonly assumed that there are city types, rural types, town attitudes, and farm attitudes. The rural and the city "mind" are other expressions of this viewpoint. Durkheim's sacred and secular, and Tönnies' *Gemeinschaft* and *Gesellschaft* are the formal models of this type of thinking. This polarized patterning has been grafted onto

the development of rural and urban sociology in America so that today each discipline is concerned with the study of social life and the meaning it has for village and farm people, town and city dwellers.

Currently the typological approach enjoys considerable popularity with a few anthropologists and many sociologists; Wirth,⁵² Zimmerman,⁵³ Taylor,⁵⁴ Loomis,⁵⁵ Heberle,⁵⁶ and Angell⁵⁷ to name but a few who are interested in studying communities and "typing" them within some larger theoretical frame of reference, such as the folk-urban continuum of Redfield⁵⁸ or the stability-instability construct which the Division of Farm Population and Rural Life followed in its study of six rural communities.⁵⁹

Is there anything new in the typological approach which was not present in the works of the social scientists who have used

⁴⁸ Louis Wirth, "Urbanism as a Way of Life," *American Journal of Sociology*, XLIV: 1-24, July, 1938.

⁴⁹ Carl C. Zimmerman, *The Changing Community*, New York, 1938, esp. 1-28.

⁵⁰ C. C. Taylor in Ralph Linton (editor) *op. cit.*

⁵¹ Rudolf Heberle, "The Application of Fundamental Concepts in Rural Community Studies," *Rural Sociology*, 6: 203-215, Sep. 1941.

⁵² Charles P. Loomis, *Social Relationships and Institutions in Seven New Rural Communities*, U.S.D.A. Social Research Report No. XVIII, Washington, D.C., 1940.

⁵³ Robert C. Angell, "The Social Integration of American Cities of More than 100,000 Population," *American Sociological Review*, 12: 335-342, June 1947.

⁵⁴ Robert Redfield, "The Folk Society," *American Journal of Sociology*, LII: 293-311, Jan. 1947.

⁵⁵ *Culture of a Contemporary Rural Community*, *Rural Life Studies*, Numbers 1-6, United States Department of Agriculture, Washington, D.C. Number 1. Olen Leonard and C. P. Loomis, *El Cerrito, New Mexico*, 1941; Number 2. Earl H. Bell, *Sublette, Kansas*, 1942; Number 3. Kenneth Macleish and Kimball Young, *Landaff, New Hampshire*, 1942; Number 4. Walter M. Kollmorgen, *The Old Order Amish of Lancaster County Pennsylvania*, 1942; Number 5. Edward O. Moe and Carl C. Taylor, *Irwin, Iowa*, 1942; and Number 6. Walter Wynne, *Harmony, Georgia*, 1943. James West, *Plainville, U. S. A.*, is essentially a typological study, but it has certain elements associated with the stratification approach in it.

⁴⁸ John Dollard, *op. cit.*, 62-97.

⁴⁹ W. Lloyd Warner, "The Society, The Individual and His Mental Disorders," *American Journal of Psychiatry*, 94: 275-284, Sep. 1937.

⁵⁰ Allison Davis, "Socialization and Adolescent Personality" *Forty-Third Year Book, Part I, National Society for Study of Education*, 1944, 198-216.

⁵¹ Herbert Blumer, An Appraisal of Thomas and Znaniecki's, *The Polish Peasant in Europe and America*, Critiques of Research in the Social Sciences: I, Bulletin 44, Social Science Research Council, New York, 1939.

comparative methods? Secondly, I wonder if the typologists are not fitting their data procrustean-like into a mold and ignoring many significant aspects of the empirical world. Moreover, they appear to reify their types. I am thinking here of Zimmerman's "real" and "nominal" communities.⁶⁰ A fourth point worth noting is the obvious incongruity between the descriptions of social organization in the same type of area as seen by different writers. I am thinking particularly of the slum as depicted by Zorbaugh⁶¹ and by Whyte.⁶² Swanson has worked in this area, and he may have some comments upon this and other points relevant to the emphases of the typologists.

By way of appraisal, it may be said that students of the community have amassed a greater store of facts about American life, as it is lived in specific places in our era, than any other group of social scientists. I believe that it is safe to assert they have collected more data, on more aspects of life, its motives, goals, achievements, and failures, than have been assembled in any other comparable period in world history. This is no mean achievement. By so doing, they have learned many things about the structure and operation of our society on the specific everyday community level. They have also formulated some generalizing constructs designed to explain the empirical facts which they have observed and/or discovered. Unquestionably, students of community life have made notable steps in the direction of realizing the ideal of an eventual nomothetic science. However, the meticulous observation of facts, although absolutely necessary for the development of a scientific sociology, has often been marked by a tendency to skip some of the steps necessary to reach the goal of a science based on empirically derived laws.

If the maturity of a scientific discipline is measured by the clarity of its terminology,

⁶⁰ Carle C. Zimmerman, *op. cit.*

⁶¹ Harvey Zorbaugh, *op. cit.*, 127-158.

⁶² William F. Whyte, *Street Corner Society*, Chicago, 1943, and "Social Organization in the Slums," *American Sociological Review*, 8: 34-39, Feb. 1943.

the precision of its concepts, the articulation of hypotheses and constructs with general theory, and the congruity of theoretical generalizations with empirical observations,⁶³ then the field of community, within the larger sociological domain, lies on a very low level of scientific development. Sociologists are neither sure of what they mean by such basic terms as "city," "community," "neighborhood," and "ecology," nor are they consistent with one another in their usage. Ordinarily these terms are used in such loose and indefinite ways that the reader is unable to determine precisely what a given author means by the words he uses. The term "community" is defined in at least three different ways in current literature: 1. as a form of group solidarity, cohesion, and action around common and diverse interests; 2. as a geographic area with spatial limits; or 3. a socio-geographical structure which combines the ideas embodied in 1. and 2. Clearly it cannot be all three. Kollmorgan and Harrison recently subjected the concept "community," as used by rural sociologists, to a searching analysis.⁶⁴ Their criticism, however, would apply with equal force to urban sociologists, ecologists, and social anthropologists. In view of this, every student of social life in local areas must be careful of the way he uses his terms. Thus, one of the basic problems students of the community should face is *clarification of terminology*. The first step in the solution of this problem is the definition of terms; and the second is the limitation of the terms, as defined, to empirical phenomena to which they are applicable.

Further development of theory in this field appears to be dependent upon greater precision in methodological procedures—definition of terms and constructs, classifi-

⁶³ Florian Znaniecki, "Controversies in Doctrine and Method," *American Journal of Sociology*, L: 514-521, May, 1945; George A. Lunberg, *Social Research*, New York, 1942, 1-30, 84-86; Frederick J. Teggart, *Theory of History*, New Haven, 1925, 153-165.

⁶⁴ Walter Kollmorgan et al, Harrison, "Search for the Rural Community," *Agricultural History*, 20: 1-7, Jan. 1946.

cation, formulation of hypotheses with dependent theorems and postulates, and the empirical test of such hypotheses and constructs—rather than continued collection of facts about life in a local area without regard to the objectives to be achieved. In conclusion, I believe that the time has come when investigators should think primarily in terms of the development of a coherent body of theory about the community, and should use this idea rather than the collection of facts for facts' sake as their frame of reference when they go to the field.

DISCUSSION

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Professor Hollingshead has posed several questions to be answered by human ecologists. In slightly altered form and order four of them may be stated as follows:

- (1) What aspects of the human community are attributable to cultural and what ones to non-cultural factors?
- (2) How should human ecology be defined and delimited as a field for scientific investigation?
- (3) Should human ecology be viewed as a field of sociology, or as a separate field in either the biological or the social sciences?
- (4) What ecological principles or concepts seem valid today?

I shall center my part of this discussion around these four questions and shall give brief but specific answers to or comments on each of them.

The first question, which involves the dichotomy of cultural versus non-cultural aspects of the human community, stems from: certain statements made by Park, possibly misunderstood, but often vigorously attacked by his critics, to the effect that human ecology studies the "natural" non-cultural as contrasted with the cultural aspects of areas. I wish to state as vigorously as possible that this dichotomy of cultural versus non-cultural has no value whatsoever either for delimiting the field of human ecology or in making ecological studies of human communities. Sociological students of human ecology know that most ecological relations among men necessarily involve culture. When men compete for ecological position or

for limited resources of the environment they ordinarily reflect cultural influences in three ways: (1) in the specific forms of inner needs they strive to satisfy, (2) in the specific meanings given to the environmental resources they utilize, and (3) in the specific knowledge, skills, and tools they use in the ecological processes of adjustment. Therefore, any implication that human ecology deals with non-cultural as contrasted with cultural aspects of the human community is incorrect and misleading.

The next two questions, dealing respectively with the definition of human ecology and its relations to other academic disciplines are so closely related that they can best be answered together. Inclusiveness of definition necessarily affects the conception of logical relations between human ecology and traditional disciplines.

I have no *logical* objection to the inclusive definition given in the *Dictionary of Sociology* which identifies human ecology as "that branch of science which treats of the reciprocal relations between man and environment." This broad definition has value in emphasizing adjustmental relations between man and environment as the central distinguishing feature of human ecology; but at the same time the inclusiveness of this definition logically necessitates the extension of human ecology into several traditional academic disciplines, both social and biological. The great variety of data, techniques, concepts, and hypotheses necessary to investigate all aspects of the relations between man and environment makes it impracticable for any research scientist to master all of them. *Practically*, therefore, the broad field of human ecology thus defined must continue to be split among traditional divisions of science.

For purposes of the present discussion, however, I shall neglect all biological studies, especially those that deal with single individual organisms, and shall consider only group adjustments to environment such as legitimately fall within the range of social science. Furthermore, in order to simplify this discussion still more and to point it specifically toward the topic of this meeting, namely *Community Research*, I shall restrict my remarks entirely to studies of the spatial structure of communities. The community is one of the major types of areas studied by human ecologists, and its spatial pattern is one of the more obvious aspects of group adjustment to environment.

Even if human ecology were to be limited to

studying the spatial structure of communities it should be conceived as a marginal field of social science. It would include some aspects of geography because varying conditions of topography directly influence the spatial pattern; some aspects of history because the inertia of past settlement influences contemporary structure; and some aspects of sociology because interrelations among men play a fundamental part in determining the spatial pattern of most human communities. If, therefore, human ecology seeks to give a relatively complete explanation of the spatial structure of communities it logically must be conceived as a marginal and perhaps as a separate field of social study.

This integrative, synthesizing conception of human ecology which aims at relatively complete explanation of areal spatial structure, seems quite valid. Moreover, it probably is the most useful point of view that has been developed, at least in so far as a given concrete community is concerned, because it affords an indispensable basis for sound areal planning and it gives valuable guidance to private owners and utilizers of space. Acceptance of this point of view does not deny, however, the need for other narrower lines of theoretical ecological research within the framework of traditional social disciplines, especially in geography and sociology.

Specialized fields of ecological research in geography and sociology seem justified when one considers the analytic as contrasted with the synthesizing function of science. In its analytic procedures science always makes abstractions from reality. Such analysis never, in itself, offers a complete explanation of any concrete phenomenon. Analytic procedures always strive to neutralize most aspects of concrete reality, either by abstraction or by holding them constant, meanwhile seeking to establish generalizations about repetitive relations between those few factors that have been selected for intensive study. It is my belief that these narrower analytic studies can best be pursued within the specialized frame of reference of traditional disciplines. I believe, therefore, that separate specialized branches of human ecology should be maintained in geography and sociology. My own preference is that the former should be labeled as "human geography," the latter as "interactional ecology."

The basic function of the specialized sociological (interactional) ecologist in community

research is to discover those repetitive regularities in the areal structure and distribution of human phenomena which depend on interactions among men. These interrelations occur chiefly at the "sub-social" ecological level when men utilize limited environmental resources on which others depend, or when they utilize limited space which others wish to use or control, or when they alter ecological distances by affecting the efficiency of transportation or by developing new sources of supply. In seeking repetitive regularities in community relations the interactional ecologist strives to discover what typical areal structures would emerge and what repetitive changes would occur (1) if no environmental barriers or inequalities existed within the community, (2) if culture remained constant, and (3) if the effects of historical inertia had been neutralized through careful analysis. Because he deals only with limited partial aspects of the community, the analytical interactional ecologist never presumes to offer a complete explanation of any concrete areal pattern. His contributions are, nevertheless, one essential part of making a complete explanation of areal structures possible.

Because of his knowledge about community structure and the processes of change which alter it, the interactional ecologist may help to explain the nature and distributions of various human phenomena such as population, property, institutional patterns, and social problems. Such studies should, in my opinion, be regarded as marginal between ecology and other branches of sociology, and not as constituting the heart of interactional ecology.

Let me finally list some of the concepts and hypotheses that apply to community studies and that seem valid and useful within the field of interactional ecology as defined above.

Among the valid concepts of community spatial structure I would list such ones as ecological distance, ecological position, dominance, gradient, natural area, and zone. Valid concepts of ecological process include ecological interaction with its sub-forms of cooperation and competition, and the sequential processes of aggregation, expansion, concentration, centralization, segregation, invasion, succession, migration, and mobility. Each of these concepts requires precise definition within human ecology, which definitions may differ considerably from usages in other divisions of sociology. When properly defined, however, each concept becomes a useful tool for studying repetitive regulari-

ties in those aspects of structure and change which depend on interrelations among men.

As illustrations of ecological principles, three presumably valid hypotheses that underlie community spatial structure may be suggested without elaboration: (1) the hypothesis of minimum ecological distance which states that, other costs being fixed, ecological units tend to distribute themselves throughout space so as to minimize the cost-distance* of transporting men and materials; (2) the hypothesis of median location, which states that the most efficient location for any ecological unit is at the median (weighted) of all units which are to be transported to and from it; and (3) the hypothesis of intensiveness of utilization which states that when two units compete for a common median location, that one tends to occupy it which utilizes the location most intensively. Such general principles may have great value in explaining the spatial structure of a concrete area but, by themselves, they are not sufficient to give a complete explanation of it.

DISCUSSION

Harold F. Kaufman
University of Kentucky

As the ecological section of Prof. Hollingshead's paper has been treated, I shall confine my remarks to the section on social structure and more specifically to the discussion of community stratification. Several basic questions are raised and criticisms made concerning current research on social stratification. These include a question as to the nature of the class concept and a criticism of "vague procedures" which have been employed in the study of stratification. Investigators who fail to demonstrate the functional relationship between class and other social factors and processes, and those who assume class groupings are obvious to all are also criticized.

Is the term *social class* a "heuristic device" or "an entity in its own right"? This raises the question concerning the nature of scientific concepts. Such a question deserves attention because much confusion in sociological thinking results from the lack of an explicit and clear-cut notion on this point. Conception is the classification and symbolization of a series of perceptions. The world of events is given unity and

meaning through the concept which is an abstraction of the common element in a series of perceptions.

The nature of the concept is determined by its relation to the world of events or "the something out there." A concept is a product of (1) the knowing operation and (2) a series of events. Notions concerning the nature of scientific concepts vary in terms of the emphasis placed on the above two elements. For the discussion both the knowing operation and the event is important (modified positivism). Thus the concept is essentially a construct; it is scientifically useful if it (and related constructs) gives a pattern of experience which is concise, internally consistent and predictable.

The position taken in the paper under discussion that social class is a construct is, therefore, accepted, but it should be hastily added that the class concept is not unique in this respect. Some writers have distinguished between "real" and nominalistic notions of class. Except for the fact that they regard their definitions of class as the "real" ones, the basis for this distinction is not entirely clear. Perhaps a concept is regarded as "real" or "an entity in its own right" if its empirical referent is identified with ease. In the case of a social grouping this would mean that the interaction pattern or other characteristics by which individuals could be identified as belonging to the same grouping were obvious to the layman. Although useful, ease of definition alone does not determine the value of a scientific concept. The basic question is not whether the class concept is "real" or "nominal," but (1) By what procedures is it defined? and (2) How is it related to other constructs—the content of the term?

A consideration of method of definition leads to Prof. Hollingshead's second criticism, namely, the "vague procedure" employed by some investigators in defining class. Three types of definitions have been distinguished (by the Subcommittee on the Definition of Definition of the Committee on Conceptual Integration of the American Sociological Society)—the genetic, the analytical and the identifying. One employs a genetic or an operational definition when he describes the procedures by which a concept is defined. An analytical definition is followed when a given concept is related to significant constructs or factors, and the question is answered, What is it? "Vague procedures" have been the result when an investigator has failed

* Cost is here used in the broad sense to include whatever is given up, and not in the narrow sense of mere economic cost.

to make explicit his methodology or research operations.

The three methods most commonly employed in determining class rank are (1) ratings of selected judges, (2) socio-economic scales and (3) single criterion such as income or occupation. The rating method of class definition need not be vague but may be quite explicit and rigorous as is shown in Prof. Hollingshead's study of "Elmtown." The most scientifically useful procedure of class definition in any given situation would be the one that would give the highest prediction (i.e., most highly correlated with each of the factors forming the class complex), and could be employed with the greatest ease.

The third type of question raised by the paper under discussion concerns the nature or essence of the class structure. Many writers have named this "something" status but more work is needed in developing a model or ideal type class or status structure. The notion of status arises from the observation that some individuals are more respected; their behavior has greater value or prestige than have the actions of others. Status is recognized when individuals act and react toward each other as equals, superiors, or inferiors. Almost all social relationships and structures have a status aspect. Some of the factors noted most frequently by which status is recognized are material possessions, social participation, income, level of consumption, political power and morals. A person's overall or community status, however, is a composite of his statuses in the various social structures and groupings in which he is involved. Because of this status groupings or structure would very seldom if ever be identical with income, ethnic or other groupings.

Prof. Hollingshead justifiably criticizes certain investigators for assuming that given class groupings are obvious and thus no need exists for making one's research procedures and assumptions explicit. He asks, e.g., "What an American upper, middle or lower class is." Some progress toward answering this question has been made, yet much remains to be done. Certainly an investigator in describing given class groupings needs to make his method, including his position in the research situation and the universe studied explicit. Would not the characteristics of the middle class vary from rural to urban communities and from society to society? Also, is it defined as the middle grouping on an occupational, income or other type of con-

tinuum, or is it a grouping to be recognized by certain characteristics, attitudes and behavior patterns? Perhaps in a general way both questions may be answered in the affirmative, but "why" and "how much" is a research problem concerning which a priori judgment is inappropriate.

In view of the above limitations, what contributions have been made in the field of community stratification? First, class research has created considerable interest both professional and lay in understanding this important aspect of our present day society. Second, definite strides have been made in defining class operationally. Third, data are accumulating concerning the characteristics of various class groupings.

In conclusion a comment on typology may be in order. Class or status groupings are only one aspect of community structure. A comprehensive typology must await a clear-cut definition of significant structural variables—status and others. The somewhat impressionistic typology thus far has resulted from the use of ill-defined and implicit criteria. It is not only necessary to clearly define significant variables but it is also desirable to interrelate them so as to discover the most meaningful clusters and types. In this respect the development of folk-urban types is promising.

DISCUSSION

G. E. Swanson
University of Chicago

Professor Hollingshead's paper makes it evident that students of ecology and the community share problems faced by other sociologists. They find difficulty in defining their specialty, in clarifying the concepts they use, in determining the relative importance of given areas for future investigation, and in choosing suitable techniques for their research.

The focus of "human ecology" is a subject of long dispute. The concept "community" is not much clearer. We find it identified with groupings that range from neighborhoods to regions. In actual practice, students seem to be caught up in the awareness of the substructuring present: of status relations, classes, voluntary associations, ethnic groups, family systems, occupational groups, age categories, and of others that can be studied as *parts* of a given *society* without too much concern about identifying smaller local units. This may be prophetic. Perhaps the

next decades will bring still fewer studies of the "community" as such.

Like other sociologists, the human ecologists and the students of the community are working to develop their concepts. Here, too, they share the growing enthusiasm for one variety of operationalism as an answer to criticisms that their ideas are vague, imprecise, unstandardized, and subjective. Objects are equated to the steps employed in their "measurement," or to the quantitative result of that measurement. This trend continues apparently uninfluenced by such critiques as those of Franz Adler,¹ Gordon Allport,² Herbert Blumer,³ Robert McIver,⁴ Ethel Shanas,⁵ and N. S. Timasheff.⁶

The concept of "social class" is a case in point. When the first volume of the *Yankee City* series⁷ was published, C. Wright Mills⁸ pointed out that Warner and Lunt gave a catch-all definition to the term "class," and did not describe their method for stratifying the population of Newburyport. A. B. Hollingshead⁹ and Harold Kaufman,¹⁰ among others, constructed indices of class. Recently, W. Lloyd Warner has provided a similar "scale" for stratification. From each of these we obtain a numerical index. We are told that these numbers are points on a continuum of status or class as the case may be. "But what is the nature of

the status or class continuum on which these are the points?" we ask. The answer seems to be, "It is the continuum on which these are the points." The index ranks status, and status is defined as that which the index ranks.

As one commentator¹¹ points out, this procedure is an act of reduction at the expense of the concept's empirical nature with which one is concerned. It can only be successful as a definition, he continues, when the item referred to is essentially quantitative in nature. The method does isolate a stable content, but that content is an unknown quantity. Having an unknown character, the item cannot be studied. It does not stand for a problem to be investigated. It cannot be generically extended. The quantitative result only becomes intelligible when translated into a definition of a kind that is different from a "measurement."

Now it is possible that once we determine what we mean by class or status, we shall find some indices of those objects of study which may be used for stratifying a population, but the construction of such indices prior to the identification of the concept does not tell us anything about the nature of that concept.

Whether statuses and classes are entities that can be separated out in a clean-cut fashion remains to be seen. Certainly there are continua of economic position, of power, and of prestige in society, and it seems reasonable that indices of position on those continua can be constructed. Those in existence already differentiate significantly among varied ways of life. They are valuable in this respect. They are dangerous if they become substitutes for more adequate explorations of the reality they represent, or if they turn attention away from such explorations. At best, they are indices of something, and their usefulness derives from our knowledge of that underlying reality.

Professor Hollingshead suggests that some typologists are fitting their data into a "procrustean mold and ignoring many significant aspects of the empirical world." One can certainly find instances of that practice. However, as Hollingshead states, by their very nature, types are not intended as exact reproductions of the empirical world, but as simplified generalizations about selected features that are useful for studying some problem, for prediction and for eliminating the mass of particulars found irrelevant to a specific question. There is probably no community in which life actually exists

¹ Franz Adler "Operational Definitions in Sociology," *The American Journal of Sociology*, LII (1947), 438-444.

² Gordon W. Allport "The Psychologist's Frame of Reference," *Psychological Bulletin*, XXXVII (1940), 20-21.

³ Herbert Blumer "The Problem of the Concept in Social Psychology," *The American Journal of Sociology*, XLV (1940), 707-719.

⁴ Robert M. McIver *Social Causation* (Boston, 1942), pp. 157-158.

⁵ Ethel Shanas "Comment," *The American Journal of Sociology*, XLVIII (1943), 489-491.

⁶ N. S. Timasheff "Definitions in the Social Sciences," *The American Journal of Sociology*, LIII (1947), 201-209.

⁷ W. Lloyd Warner and Paul S. Lunt *The Social Life of a Modern Community* (Yankee City Series, Volume I), (New Haven, 1941).

⁸ In a book review in *American Sociological Review*, VII (1942), 263-271.

⁹ A. B. Hollingshead "Selected Characteristics of Classes in a Middle Western Community," *American Sociological Review*, XII (1947), 385-395.

¹⁰ Harold F. Kaufman *Prestige Classes in a New York Rural Community* (Memoir 260, Cornell University Agricultural Experiment Station), (Ithaca, 1944).

¹¹ Blumer, *op. cit.*

as described in Wirth's¹² discussions of urbanism. He has chosen the salient features of a way of life. He has shown their interrelations. He has pointed to the direction in which change is occurring.

To me, the problem of the urban slum as a type is of particular interest. Zorbaugh's¹³ description arises from studies of the inhabitants of a rooming-house area. Whyte¹⁴ lived in, and described, one Italian residential area in Boston. These sections are quite different in structure. The resultant descriptions reflect those differences. Earlier this year, I spent some time in an area usually described as one of the worst slums of an eastern industrial city. There was evidence of the existence of the patterns described by both Zorbaugh and Whyte. In addition, it became apparent that most of the citizens were not isolated, mobile individuals, nor were their lives organized around professional vice and gambling. Their value system was very like that one might expect to find in what Warner calls a lower-middle class community.

Perhaps typologies of the slum are premature. Perhaps such attempts need to be approached with new perspectives. In any case, the empirical discrepancies with our present constructs pose a vital problem for those who wish to link such phenomena as delinquency or mental disorders to types of community life.

In looking back over the development and present status of the field of the community, we find many studies of particular areas and of specific problems—studies that are rich with intimate details of human living. But these pictures are largely still-lives.

It is a truism that all societies change. It is a commonplace that our own way of life is now characterized by widespread and profound changes exhibited in the loosening and disruption of traditional communal bonds and in the increase of personal freedom. In this changing social medium, human individuals and human groups meet problems for which there are no established solutions. New experience, and dissatisfaction with the established order, abound. It is this evident process of change that tends to escape our studies.

Part of the difficulty may lie in our use of conceptual schemes that are designed to picture a relatively stable way of life. I am thinking now of such concepts as status, class, institutions, and structure. By their nature, these ideas make it difficult to see the process of change, to raise significant questions about the nature of that process, and to plan research designed to answer those questions.

However, the difficulty may lie still deeper in our theoretical schemes. There is a tendency among many students to equate the concept of society with that of culture. The resultant blurring of the shifting, dynamic processes of interaction, that underlie the formal structure of accepted rules and practices at any given time, leaves us unable to account for the changes that appear in that formal structure. It focuses our attention in other directions. Often, we add to this the assumption that man is a kind of neutral creature buffeted about by the natural environment, by society, and by culture. Yet, the idea that man is the active agent in changing his society and culture is not new in sociology. Sumner, for example, described the societal selection of the mores as a process through which men act collectively to change their world. It is characteristic of many, perhaps most, contemporary students that they ignore this description and concentrate, instead, on a picture of the traditional rules molding human behavior.

This problem of the nature and process of social change deserves a concerted research effort from students of the community. There are some suggestive, and some inadequate, publications by specialists in many fields dealing with the question. Little has been done to systematize and test these hypotheses.

Certain studies appear as among those appropriate for students of the changing community. I have in mind the study of communications and of communication, of the rise of new personality types, of the developing phenomenon of mass behavior, of the dissatisfactions with current value systems, of the voluntary associations and the social movements that symptomize the areas in which change is occurring and the directions in which it moves, of those situations that people regard as crises, of shifts in the process of socialization, of the growing opportunities for individuals to have somewhat unique experiences inside the social structure, of the appearance of isolation and of loneliness and of anomie. These, and countless other problems,

¹² Louis Wirth "Urbanism as a Way of Life," *The American Journal of Sociology*, XLIV (1938), 1-24.

¹³ Harvey W. Zorbaugh *The Gold Coast and the Slum* (Chicago, 1929).

¹⁴ William F. Whyte *Street Corner Society* (Chicago, 1943).

can be studied in a manner to reflect back upon the nature of social change.

The possibilities of accounting for the changing community and for predicting the nature of communities in the future appear to lie in this direction. The development and testing of a suitable conceptual framework is a prerequisite for realizing those possibilities.

DISCUSSION

Walter Firey
University of Texas

Dr. Hollingshead's paper had raised a number of interesting problems concerning the status and prospects of community and ecological research. In line with the plan for this symposium I shall confine my own remarks to just a few of these problems, namely those more strictly ecological in character. In so doing, however, I do not wish to lose sight of Hollingshead's point that, in a very real sense, the development of ecology as a field of inquiry has been but a phase in the broader development of community theory and research. Indeed, I would go even further and suggest that the future prospects for ecology are going to vary in direct relation to its rapprochement with general community theory and research.

Implied in this statement is the idea that ecological processes are at every point corollary to, and contingent upon, social and cultural processes operating at the community level. One cannot, in other words, dissociate the ecological from the social or cultural—the "biotic" from the "moral"—if one would have a valid ecological theory. It is precisely through its false dichotomy between these two components that ecology has come to its present impasse, in which it lacks both logical integration and empirical adequacy. The whole trend of research and criticism during the past decade has emphasized the untenable character of this dichotomy.

A number of implications follow from this position. Consider the problem posed by Hollingshead in his question concerning the relative influence of the physical and biotic environments as against the cultural environment. He asks: "What aspects of the human community as conceived by the ecologist are attributable to non-cultural and what ones to cultural factors?" In terms of the position just outlined, the answer to this question is clear. There can be no environment apart from that which is de-

fined into being by social and cultural processes. Indeed, it is *mal à propos* to speak of an environment which, in the words of George H. Mead, is "just there." For as Mead has rather aptly put it, "The community as such creates its environment by being sensitive to it." (*Mind, Self and Society*, p. 250. Italics mine.) Such a viewpoint in no way denies the causativeness of non-cultural factors; it merely indicates that their causativeness is mediate, that cultural factors intervene at every point between non-cultural factors and the human community.

This reasoning is quite consistent with Hawley's definition of ecology, which I find rather useful, *viz.*, that "Human ecology . . . (is) the study of the development and the form of communal structure as it occurs in varying environmental contexts." (quoted in Hollingshead, *supra*). But from this it does not follow that these "varying environmental contexts" are metaphysically real things which are "just there." Rather they are social and cultural constructs. To be sure, natural phenomena and objects, such as rivers and mountains, do have an objective reality. But, however real they may be, these phenomena do not become "environments" until they are defined as such by particular societies having particular cultures which render those phenomena behaviorally meaningful.

This reasoning leads us to another of the questions raised by Hollingshead. He asks: "What ecological constructs—principles or concepts—do you consider to be valid today?" A great many of the ecological constructs which we have inherited and with which we work in our research have had real heuristic value. Such constructs as "time-cost distance," "competition," "natural areas," and "biotic community" have served a useful function by orienting our research activities into theoretically significant directions. To be sure, the resulting data have on occasion compelled us to redefine some of these concepts (e.g., "natural area") and in some cases to even discontinue their use (e.g., "biotic community"). But that is the way in which science develops. In physics the concept of "energy" has come to mean a very different thing than it did twenty-five years ago. Concepts have a certain instability of reference, being subject to change through the demands of empirical data, through the varying formulations of problems, and through the strain toward logical consistency in theoretical schemes.

It is therefore difficult to say just which ecological concepts are valid today. From one point of view, very few of them are valid, if one has only in mind the logical requirements of systematic theory, namely, the formulation of strictly homologous concepts or variables; the arrangement of these variables into propositions which specify determinate relationships between the variables; and, finally, the logical deducibility of each of these propositions from any combination of two or more other propositions in the systems. In these respects ecological constructs are today quite inadequate.

From another point of view, however, a great many of our ecological constructs have a real pragmatic validity, insofar as they represent tentative tools by which we apprehend a significant segment of social reality. Whatever ecology is to be fifteen years from now, it will certainly show a direct continuity with the ecology of today, and its concepts too will have a generic relationship with those of today.

The concept of "competition" may be a case in point. In orthodox ecology the competitive process was viewed as something "sub-cultural" and automatic. It was posited as the dynamic regulative process underlying the spatial and temporal aspects of communal structure. Now indubitably competition is an identifiable process that may be observed in all societies, whatever their culture. But what people compete for, the terms on which they compete, the communicative and exchange channels through which they compete, and the manner in which they routinize and organize their competition—all these are culturally defined. It is putting the cart before the horse to ascribe causal priority to competition and to regard the cultural context as merely epiphenomenal. The truth is more nearly the other way around. Accordingly the concept of "competition" is sure to be redefined as ecological research and systematization progress. One can grant that "competition" is indeed a "valid" concept, but, like all other scientific constructs, it is tentatively rather than immutably so.

The same reasoning applies to such ecological generalizations as: "The market integrates competition"; or "the metropolis dominates the rural hinterland." Certainly these are "valid" propositions, but only to a first approximation. We must go further and ask: What forms does competition assume in different social structures? What are the cultural prerequisites for the very existence of certain kinds of markets

(the department store, for instance)? How is dominance mediated by social structures such as class distinctions, ethnic cleavages, and differential institutional participation? Only insofar as such social and cultural variables become integrated with ecological theory can these propositions be consonant both with empirical data and with the logical requirements of a closed theoretical system. The validity of these propositions is therefore real, but only approximately so.

Let me turn briefly now to the third of Hollingshead's points, that concerning the status of ecology as a formal discipline. There seems to be, I think, an increasing acceptance of the view that ecology is a marginal or borderline discipline. This position has been developed most fully by Gettys and Quinn, both of whom liken ecology to such other marginal fields as social psychology, social anthropology, and physical chemistry. Such a viewpoint comports very well with the present-day trend toward social science unification and the breaking down of departmental barriers. Certainly the adequacy of ecological analysis will be increased insofar as it succeeds in assimilating the data and the insights of geography, economics, and geopolitics as well as those of sociology.

In this fusion of research data from fields so divergent in their traditional preoccupations and approaches, the role of systematic theory will be paramount. The time has indeed come, as Hollingshead reminds us, when ecological research, and community research in general, must be increasingly oriented toward the testing of fundamental hypotheses and the gradual development of a coherent body of community theory.

DISCUSSION

Amos H. Hawley
University of Michigan

Dr. Hollingshead has raised a number of pointed questions which I shall attempt to answer in a discussion of the role of human ecology in community research.

Ever since sociologists turned from a preoccupation with concepts to seek an empirical basis for their theories the community has come increasingly into the focus of their attention. While it may be impossible to set down the historical reasons for this concentration of interest, it is in accord with some very sound methodological considerations. Perhaps the most important of these is that for a great number

of social phenomena the community is the least reducible universe of cause and effect relations. Any smaller social unit would fail to contain a sufficient number of relevant variables; any larger unit would prove unmanageable. A second advantage attached to the community as an object of study is that it is the most nearly comparable unit part of different societies. Hence the community provides a valuable basis for many types of comparative studies.

Needless to say, human ecology has played an influential part in directing the interests of sociologists to the community. The organismic position of human ecology, its emphasis on the importance of locale and the frictions of space, its encouragement of field research on specific problems of structure, and the vast amount of research product it has inspired have all contributed to that end. In addition to its general effect on the orientation of sociology, human ecology has presented us with a large mass of descriptive materials on the spatial pattern of the community. While many of the descriptive formulations that were made, e.g., the concentric zonal hypothesis, natural areas, succession, etc., must be regarded as first approximations to a dependable knowledge of spatial distributions of community elements, they have not yet been supplanted by more adequate conceptualizations. They are in wide use today by sociologists, urban geographers, city planners and others concerned with spatial configurations of communities.¹

Too little attention, however, has been given to the implications of spatial patterns. Apparently spatial analysis, in the minds of many people, is a means to practical but not theoretical ends. But let us ask the question: What is it that is assumed to be patterned in space? Surely it is not merely the physical components of settlement. Nor does it seem likely that the incidences of delinquency, mental disorder, broken homes and the like are all that is involved. I hardly believe that simple "Rorschach blots" on the landscape interest us as sociologists. Rather I suggest that what is assumed to be spatially patterned is community structure, that is, the organization of relationships by which men sustain themselves. There seems to be no other theoretical justification for a concern with space. On the other hand, there is

much to recommend such an assumption. Men live close to the ground, in a space-time universe, as it were. It might be expected therefore that distance, however it may be encountered, would affect the number, the periodicity, and even the content of interrelationships. In short, given the assumption that community structure manifests itself in a spatial pattern, we have a ready means of measuring and of communicating the complexities of man's collective life.

Granted this, it now appears that the possibilities of spatial analysis as an approach to knowledge of community structure have been virtually exhausted. At any rate, while our knowledge of distributions has been progressively refined, our knowledge of structure has not advanced correspondingly. One may fairly argue, in fact, that ecologists have worked from the wrong end. Perhaps they would have been wiser to have first established the principles of community structure before undertaking to determine the expression of structure in space. What was adopted as an assumption might better have been regarded as an hypothesis. Still, in view of the state of sociological thinking twenty-five years ago there may have been no practical alternative to the course that was followed. We could debate this question at considerable length, but the outcome would add nothing to the progress of knowledge. The point I wish to make is simply that it is time ecologists devoted their efforts to the study of community structure.

We should not slight the earlier attempts of ecologists to examine the organization of the community apart from its distributed aspect. R. E. Park's conception of biotic, economic, cultural, and moral levels of behavior, though quite unsatisfactory, was a gesture in that direction.² Likewise the frequent use of concepts such as symbiosis, division of labor, equilibrium, and dominance are indicative of a similar interest. On occasion, albeit all too infrequently, ecologists have carried their interest in structure into empirical work. That is witnessed, for example, by studies of the types of service agencies in cities and of the relation of service agencies to the size and composition of popula-

¹ For a recent evidence of this see Robert E. Dickinson, *City, Region and Regionalism* (London: Kegan Paul, 1947).

² "Human Ecology," *The American Journal of Sociology*, XLIII (July, 1936), 1-15. See also the same author's "The Urban Community as A Spatial Pattern and A Moral Order," in E. W. Burgess (ed.), *The Urban Community* (Chicago, 1926), 3-18.

tion.³ One important consequence of this preliminary work has been to reveal that time and space are not the only frictions that operate in relationships; there are others in the form of the population requirements of different functions, the dependence of one function upon another, the necessity that all functions be coordinated, etc. But as I previously stated, very little has been accomplished in the development of principles of community structure.

Now, as it happens, community structure may be attacked from a variety of viewpoints including human ecology, social anthropology, and other sociological specialisms that are not so explicitly named. Human ecology differs from its kindred approaches in its statement of problem and in its method. The ecological problem is: How does a population organize to maintain itself in a given area? This places an emphasis on sustenance relations, yet sustenance relations are regarded as constituting only the general framework of the community. To find the answer to his problem the ecologist must be able to trace out the ramifications of sustenance activities and ascertain the degree to which they affect and are in turn affected by other kinds of activities. Thus the community is viewed from the ecological standpoint as an adaptive mechanism in which all of the parts have or tend to have functional relationships to one another.

The method of the ecologist is conditioned by the fact that his unit of study is a population rather than an individual. One effect of this is to limit ecology to data pertaining to factors external to the individual. Furthermore, as implied in the statement of the ecological problem, adaptation is essentially a collective function. The smallest unit part of the community that may be logically treated is therefore an association of individuals. Another effect of the concern with a population, and one which is in keeping with what has already been said, is that it forces the ecologist into a quantitative and descriptive procedure. Ecology, in other words, tends to be a morphological study of community organization. Time does not permit

further elaboration of these points. I trust that my remarks will be taken as a very sketchy characterization of the ecological point of view.⁴

The analysis, description, and explanation of community structure I take, then, to be the special task of the human ecologist. Structure, as used here, includes all that is comprised in the division of labor together with those relationships that are directly ancillary to and active in the support of the division of labor. Such a definition is obviously more comprehensive than is the social anthropologist's stratification conception. Yet it excludes interrelations that are transitory, atypical, or not pertinent to the preservation of the functional unity of the community. Unfortunately, it is not possible at this time to be more specific as to the exact connotation of community structure; that must await the accumulation of research findings.

A conception of community structure in which the division of labor occupies a large place is apt to provoke criticisms on the score of its being economic, or perhaps too restricted to "rational valuations." Some might even contend that such a conception does not pay adequate respect to the influence of culture. My response to objections of that character is that they beg the question. Our urge to cultivate a unique discipline is so strong that we are often inclined to insist upon the reality of arbitrary definitions. Actually what is economic is by the same token social. So called economic values (or data) contain a large though undetermined and doubtlessly inseparable non-economic component. As a matter of fact, much of the available quantitative data on social phenomena are in the form of economic statistics. I see no reason for rejecting such material because of the label it has acquired, nor do I find any basis for identifying a person as an economic determinist because he makes use of data of that kind. The differences between the several social sciences lie not in the facts they use but in the hypotheses they investigate.

Parenthetically, a close parallel to the alleged economic nature of ecological phenomena has found favor, not as a criticism, but in a positive statement of their character. I refer to Dr. Quinn's assertion that they are sub-social.⁵ That

³ See R. D. McKenzie, "Spatial Distance and Community Organization Pattern," *Social Forces*, V (June, 1927), 623-27; and *The Metropolitan Community* (New York, 1933), Ch. VI. See also Amos H. Hawley, "An Ecological Study of Urban Service Institutions," *The American Journal of Sociology*, VI (Oct., 1941), 629-39.

⁴ For a more complete statement see my "Ecology and Human Ecology," *Social Forces*, 22 (May, 1944), 398-405.

⁵ J. A. Quinn, "Ecological versus Social Interaction," *Sociol. Soc. Res.*, XVIII (1934), 565-70;

idea seems to have its basis in the early contention that the structure of the community is "natural" or unplanned, from which, apparently, the reasoning moved to the nonrational and thence to the sub-social. The distinction between the "sub-social" and the "social," however, appears to be based more on normative than on empirical criteria. But the real difficulty is encountered in the assumed departmentalization of behavior. It is in that connection that the sub-social bears similarity to the uncritical use of the term "economic." Such a distinction may be created by definition, of course, and certainly a person is entitled to make whatever definitions he chooses. Yet a difference when defined should be observable. Whether the "sub-social" and the "social" are observably distinguishable categories remains to be demonstrated. In the meantime it seems best to regard all interdependencies as social.

Much the same appraisal is applicable to the claim that the view of community structure as a division of labor implies too great an emphasis upon "rational values." The differentiation of "rational" from "volitional values," as suggested by Dr. Firey,⁶ seems to be largely verbal, and it is extremely confusing. Incidentally, what is termed "sub-social," and, by implication, non-rational, by one author is regarded as "rational" by another.⁷ Whether or not such concepts are

intelligible, they belong in the context of a motivation problem and not in that of a morphological problem.

As to whether community structure is cultural or non-cultural, or is a product of influences of one order or the other, I am unprepared to say. It seems fruitless to even attempt a separation of cultural from non-cultural factors, at least until we can agree on a precise meaning for the term culture. At present we appear determined to extend its meaning to everything touched by man with the result that the concept has become so all-inclusive as to have lost most of its discriminative utility. It may be worth noting, however, that culture is one name for that which we are trying to explain.

All of these objections and the distinctions they have given rise to impress me as merely a play upon words. If Dr. Hollingshead's assertion that human ecology has lost popularity for a decade or more is correct, as I think it is, that probably is partly a result of the verbal confusion which has gathered around the point of view. I prefer to strip away all of the debris and to start afresh with a few clearly stated hypotheses and a minimum of presupposition about the nature of the data. I know of no ecological concept which does not stand in need of qualification. The real and significant heritage that we have, it seems to me, is a problem and a working point of view. The loss of popularity promises to be a healthy condition, for it means that the band-wagon has passed on and serious students may work in relative calm on the important problems of community structure.

and "The Nature of Human Ecology—Reexamination and Redefinition," *Social Forces*, XVIII (Dec., 1939), 161-168.

⁶ *Land Use in Central Boston* (Cambridge, 1947), 34-38.

⁷ Walter Firey, *ibid.*, 13ff.

THE POSITION OF SOCIOLOGICAL THEORY*

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OBJECTIVE AND APPROACH OF THE PAPER

FOR THE present purpose it seems best not to attempt a critical survey of relatively recent contributions to sociological theory. This has been done sufficiently recently in several places; for instance, the Symposium *Twentieth Century Sociology*, edited by Gurvitch and Moore,

Merton's paper in the 50th anniversary issue of the *American Journal of Sociology*, the excellent paper by Edward Shils on American Sociology in the British publication "Pilot Papers,"¹ and more sketchily by Barber and myself in the January 1948 issue of the *American Journal of Sociology*.² This decision is further justified by the fact that there have been relatively few publications

* Paper read before the annual meeting of the American Sociological Society, New York City, December 28-30, 1947.

¹ September, 1947.

² January, 1948.

of first rate theoretical significance in recent years. This is of course, largely a consequence of the war and of the fact that so many people in the field have been preoccupied with work outside their normal professional interests. Rather than attempting such a critical survey, it seems more fruitful to present the outline of a positive orientation to the problem of sociological theory and a broad program for its development over the next few years. This is a wholly tentative attempt but can, it is hoped, serve as a focus for discussion and clarification of the fundamental methodological and theoretical problems involved. It seems to me that the time is ripe for such an attempt to deal with theory as a common task of the theoretically interested members of the professional group rather than with "theories," the critical discussion of the work of the conceptual schemes of a variety of different people. The time when the most important fact about theory in our field was its division into warring schools or the personal systems of individuals is, if not already completely passed, in my opinion, passing. We are in a position to agree on certain broad fundamentals of such a character that the large number of people who accept them can be working on common premises and, though their individual interests and contributions will differ and will cover a wide range, there is every prospect that they should converge in the development of a single major conceptual structure. It is as an attempt to assist in the implementation of this prospect that I should like to see this paper considered.³

³At the outset it seems appropriate to take cognizance of two of the points raised by Professor Merton in his discussion paper, since these were addressed to the opening section of this paper.

First, I should like to express the heartiest agreement with his remarks about the relation between theory in contemporary sociology and the history of sociological theories. The latter have been predominantly ambitious, highly generalized syntheses created by individuals. Generally speaking, as total systems they have not proved usable by the contemporary research social scientists, and those smaller elements of them which are useful have for the most part become incorporated into more recent work in more usable form than the original. Hence as part of generalized training for sociological re-

A FEW BASIC POSTULATES

It seems possible to lay down a few fundamental propositions which can serve as general guiding lines for the more technical task of building a systematic treatment of sociological theory. I shall try to state these in sufficiently general form to leave room for a variety of different approaches in working out their implications in considerable detail. The following five seem to me the most essential of these basic postulates:

A. Systematic theory itself is of fundamental importance to any science. There is, of course, no intention to deny that work which can be quite legitimately called scientific can develop without systematic theory. Morphologies, classifications and empirical generalizations of various sorts have played

search, too much time devoted to sociological theory in this sense cannot be very profitable. This point is a direct extension of my own statements of the changed orientation of the study of theory.

The second point, that the emphasis of the near future should be on "theories" rather than on "theory" raises more complex issues. It is perhaps well here to anticipate one of the closing statements of the present paper; namely, that it is not meant to present a system of theory but rather a program for the development of such a system. As such, it should be considered precisely as a framework within which the particular theories to which Merton refers can be developed. It is my contention that the time has passed when individual theories must be so particularistic that they must lack the common foundations which are necessary to make them building blocks in the same general conceptual structure, so that *theoretically* the development of our science may, to a degree hitherto unknown, become cumulative. The working out of such a framework is itself an essential and fruitful task. But it is quite true that there is danger of repetition of the old fallacy of premature closure of a theoretical system. Comte or Spencer all over again.

A crucial implication of the relation of particular special theories to such a generalized framework lies in the importance for every special theory of placing it in the framework of its role in a functioning system. To derive the immense benefits of this, a concept of system and its continual use are essential. But for this to be available it is not essential that every theoretical problem should be solved. On the contrary, the concept of system functions as a heuristic device to guide the formulation and empirical solution of such problems. It is, that is, a necessary basis for the most fruitful program of work in special theories as advocated by Merton.

very important parts in the development of a variety of sciences. The highest levels of scientific development, however, are not reached without conceptualization on the level of what is ordinarily called that of the theoretical system. The closer social science approaches to realizing this possibility, the more mature will it be considered as a science and the greater the predictive power which it will command.⁴

B. The theoretical system which is basic to sociology must be broader than that of the science of sociology itself. It must be a theory of social systems. There has historically been an important so-called "encyclopedic" view which considered sociology the synthesis of all our knowledge of human social behavior. This has the consequence of making economics, political science, etc., branches of sociology, which is not in accord either with current academic reality or with good analytical procedure. We must somehow work out a theoretical scheme which articulates our own field with others which are equally part of the same broader fundamental system.

C. The systematic theory which is most fruitful for our field must conform with the "structural-functional" type, which is current in biological theory, notably physiology. Intrinsically the type of theoretical system best exemplified by analytical mechanics is more desirable, but is not now attainable in our field either as a theoretical system as such or as a useful tool of empirical analysis over a wide range. The most notable attempt to develop such a system was that of Pareto, which with all its merits has not proved very fruitful as a tool in empirical research. The most essential point about a structural-functional theory is that by the use of structural categories it simplifies dynamic problems to the point where a significant proportion of them became empirically manageable with the observational and analytical resources we can hope to command in the near future.

D. The theory must be formulated within

⁴See qualification of this statement in Note 3 above.

what may be called the "action" frame of reference. It cannot, that is, be completely behavioristic in the sense of excluding all reference to the point of view of the actor himself and to what is imputed as belonging to his internal or subjective mental processes. This postulate is essential in order to make it possible to achieve a high degree of articulation with the motivational categories of contemporary psychology which deal with such things as attitudes, sentiments, goals, complexes, and the like.

E. The theoretical system must so far as possible be framed in terms of genuinely operational concepts. The ideal is to have theoretical categories of such a character that the empirical values of the variables concerned are the immediate products of our observational procedures. In relatively few fields of social science is any close approach to this yet possible, but with further development both on the theoretical side and in the invention of new observational and experimental procedures great progress in this direction is to be expected.

SOME METHODOLOGICAL PREREQUISITES OF THE FORMULATION OF A SYSTEM

A. *Analysis of the action frame of reference.* In order to be clear about the implications of postulate D above, it is essential to work out some of the major features and implication of the action schema. There are a variety of ways of doing this, but it has seemed on the whole most fruitful in the first instance to distinguish the orientation of the actor on the one hand, and the structure of the situation on the other. Though the situation includes both the physical environment and other persons, the point of view from which it must be analyzed for this purpose is not that of the physical or biological sciences as such, but the various types of significance of situational facts to the actor. This means that the analysis of the situation must be fully integrated with the analysis of action itself. Action, in turn, it seems convenient to analyze in terms of three fundamental modes of orientation, which may be called cognitive, goal directed, and affective, respectively. We can, that is,

"action"

have an adequate analysis of the action of the individual only so far as we understand his action and his situation in terms of his attempts to know it cognitively, in terms of the goals he is striving to achieve, and in terms of his affective attitudes toward these components and toward the situation. This is a very broad and noncommittal schema which is, in fundamental respects, in general current use.

B. *The functional prerequisites of the social system.* Somewhat analogous to the spelling out of the action frame of reference is an analysis of the functional prerequisites of the social system; that is, of a system of social action involving a plurality of interacting individuals. If the social system is to be the major unit of reference of the total theoretical schema as a whole, it must be treated in functional terms. It is assumed as a matter of empirical fact that it has either certain given characteristics as a system which differentiate it from other systems and from the non-social situation, or there is a relatively definite empirically observed pattern of change in these characteristics analogous to the pattern of maturation of the young organism. Functional requirements of the maintenance of any such pattern system or pattern line of change can be generalized to a certain degree. In the first place, of course, a social system must somehow provide for the minimum biological and psychological needs of a sufficient proportion of its component members. On a more strictly social level, there seem to be two primary fundamental foci of its functional prerequisites. One lies in the problem of order, in the problem of the coordination of the activities of the various members in such a way that they are prevented from mutually blocking each other's action or destroying one another by actual physical destruction of the organisms, and, on the other hand, they are sufficiently geared in with each other so that they do mutually contribute to the functioning of the system as a whole. The second focus is on adequacy of motivation. The system can only function if a sufficient proportion of its members perform the essential social roles with an ade-

quate degree of effectiveness. If they are not adequately motivated to this minimum level of contribution to the system, the system itself, of course, can not operate. A variety of further elaborations of the problem of functional prerequisites can be worked out from these starting points.

C. *The bases of structure in social systems.* A third fundamental methodological focus is in the nature of the structuring of the social system and the points of reference which must be considered in order to analyze that structuring. A structural category in its significance for a structural-functional system must be treated as a relatively stable patterning of the relationships of the parts, which in this connection may be treated either as component actors or as the roles in terms of which they participate in social relationships. One aspect of the structuring of the system must be what is conveniently called "institutionalization";⁵ that is, the organization of action around sufficiently stable patterns so that it may be treated as structured from the point of view of the system. This theoretical function is to enable their assumption as constants for the treatment of limited dynamic problems. There are varying degrees of institutionalization of action in different parts of the total social system.

The second major aspect of the structuring of social systems is differentiation and the patterns according to which differentiation must be studied. This is a very complicated

⁵ To avoid misunderstanding it should be clearly stated what concept of institutions and correspondingly of institutionalization is being employed in this paper. A pattern governing action in a social system will be called "institutionalized" in so far as it defines the main modes of the *legitimately expected* behavior of the persons acting in the relevant social roles, and in so far as conformity with these expectations is of strategic structural significance to the social system. An institutional pattern is thus a culture pattern (see below) to which a certain structured complex of motivations and social sanctions have become attached. It is an *ideal* pattern, but since conformity is legitimately expected it is not a "utopian" pattern. An institution is a complex of such institutional patterns which it is convenient to treat as a structural unit in the social system.

problem, and in general, treatment of it must be conceded to be in a rather unsatisfactory state. It is above all essential, however, that modes of differentiation be treated in terms of a system of categories which make different social structures comparable with each other on the structural level. Without this, any high level of dynamic generalization will prove to be impossible. The most promising lead to solving this problem seems to lie in the demonstration of the existence of certain invariant points of reference about which differentiated structures focus.⁶ Some of these may lie in the external non-social situation of action; for instance, it is possible to systematize the comparison of kinship structures by using the fundamental biological facts of relatedness through biological descent as a set of invariant points of reference.⁷ Seen in this light, social systems of kinship become as it were variations on the biological theme. In a somewhat comparable way, it is possible to treat certain elements of the structure of action in terms of the pursuit of specific goals and the social relationship complexes which grow out of that pursuit, as another set of invariant points of reference which makes structures in such areas as technology, exchange, property, organization, and the like, fundamentally comparable with each other. This is one aspect of the field which is in need of the most intensive analytical development.

THE MAIN CONCEPTUAL COMPONENTS OF THE SOCIAL SYSTEM

In the terms most immediately relevant to the sociological level of theoretical analysis, it seems convenient to classify the major conceptual components under the following four categories:

- A. The structure of the situation.
- B. The cultural tradition.

⁶ See the mimeographed document "Toward A Common Language for the Area of Social Science" by J. T. Dunlop, M. P. Gilmore, C. Kluckhohn, T. Parsons, and O. H. Taylor, for a fuller discussion of this concept.

⁷ Cf. Davis, Kingsley and Warner, W. L., "Structural Analysis of Kinship," *American Anthropologist*, Vol. 39, No. 2., 1937.

C. Institutional structure.

D. Motivational forces and mechanisms.

A. The role of the structure of the situation has already been commented on above. Here it is necessary only to say that it is essential to keep continually in mind a distinction of two major levels. The first is the structure of the situation from the point of view of any given individual actor; the second, from the point of view of the functioning of the social system under consideration as a whole. Failure to distinguish these two levels was, for instance, the primary source of the dilemma into which Durkheim fell which was responsible for most of the controversy over the group mind problem. For the most part, the categorization of this area is relatively familiar. It should only be stated that there will be refinements and shifts of emphasis as a result of further development of the more distinctively sociological elements of the total theoretical system.

B. The cultural tradition. In general the usual anthropological definition of culture is applicable in this context.⁸ Culture in this

⁸ There is, of course, no single standardized definition of culture accepted by all anthropologists. The theme of "social heredity" may, however, be considered the dominant one. The following are relatively typical samples:

Boas, Franz, *Encyclopedia of Social Science*. Culture embraces all the manifestations of SOCIAL HABITS of a community, the REACTIONS of the INDIVIDUAL as affected by the habits of the group in which he lives, and the PRODUCTS of human activity as determined by these habits.

Benedict, Ruth, 1931. The complex whole which includes all the HABITS acquired by man as a MEMBER OF SOCIETY.

Linton, Ralph, 1936, *Study of Man*, p. 288. The sum total of IDEAS, CONDITIONED EMOTIONAL RESPONSES, and PATTERNS of HABITUAL BEHAVIOR which the members of that society have acquired through instruction or imitation and which they share to a greater or less degree.

Linton, Ralph, *Study of Man*, p. 78. The SOCIAL HEREDITY is called culture. As a general term, culture means the total social heredity of mankind, while as a specific term a culture means a particular strain of social heredity.

Mead, Margaret, 1937. The complex whole of TRADITIONAL BEHAVIOR which has been de-

sense consists in those patterns relative to behavior and the products of human action which may be transmitted, that is, passed on from generation to generation independently of the biological genes. It is an exceedingly varied and complex entity, and one of the most imperative theoretical tasks is to work out careful and analytical distinctions of the different elements which make it up and of their different relations to the levels of social structure and action. The difference from the usual anthropological usage of the concept culture lies in our interest in the particularly strategic role which is played by those elements in the cultural tradition which define ideal patterns governing the action of individuals. Their strategic significance derives from the fact that these are the patterns of culture which are susceptible of institutionalization, thereby forming components of the main structure of the social system itself. It clarifies discussion of this field, it seems to me, to make clear that there is not a substantive distinction between cultural and institutional patterns, but rather it should be held that all institutional patterns are cultural. However, only some cultural patterns become institutionalized and therefore have the special significance for the social system which institutions have. The distinction is one of functional relationship to the social system.

C. Institutional structure. This is that aspect of the conceptual analysis of the social system which is of closest relevance to the sociologist. Its most generalized role in the social system has been adequately described under C in the preceding section, and will be further elaborated below.

D. Motivational forces and mechanisms. It is here that the psychological theory which is essential to the theory of social systems finds its place. It should be made quite clear that many elements of psychological theory

are of only secondary relevance to the theory of social systems. This is true of a great deal of physiological psychology and also of the more idiosyncratic elements of the theory of personality. Fundamentally the basic concern of the theory of social systems is with those elements of motivation which deal with the motivation of typical or expected behavior in social roles, and those tendencies which motivate socially significant deviance. It is important to make clear that the very common statement that psychology provides certain premises or underlying assumptions for the theory of social systems is not correct. This psychological component does not constitute a set of assumptions which lie outside the system, but an indispensable component of the system itself. It seems to me that the relation between the psychological theory of motivation of social action and the sociological aspect of the theory of social systems is closely analogous to that between biochemistry and physiology. They are both inextricably interlocked in the same body of analytical thinking in such a way that they are only analytically distinguishable.

INSTITUTIONS AS THE THEORETICAL FOCUS OF SOCIOLOGICAL SCIENCE

It has been stated above that sociology should not claim to be the encyclopedic science of all human social behavior, but should find a place among the various social sciences. In terms of this point of view, institutions constitute the logical focus of sociology. When, for instance, some of the difficulties associated with group mind problems are stripped away, I think this is what is left of Durkheim's emphasis on the study of society as a phenomenon *sui generis*. This point of view should not, however, be interpreted to mean that sociology should be confined to the formal classificatory treatment of the structure of institutions. I should prefer the formula that institutions are the focus of its interest and that almost any component of the social system which bears on the functional and dynamic problems of institutions should be defined as sociological.

veloped by the human race and is successively learned by each generation.

Malinowski, Bronislaw, *Encyclopedia of Social Science*, Art. Culture p. 621, 623. (Culture is) SOCIAL HERITAGE.—Culture is a REALITY SUI GENERIS.

From this point of view, sociological theory would, it seems to me, fall principally into the five following divisions.

A. The systematization of the study of the structural differentiation and integration of institutional patterns on a comparative basis. This includes not simply formal morphology but the theoretical analysis of the relations of structure and structural variation to the functional needs of social systems. However, relatively speaking, this could be called "pure" or "formal" sociology. Its difference from the program of Simmel or von Wiese lies in its relations to the other branches of sociological theory rather than in a difference in specific content.⁹ As has been remarked above, it is a grossly undeveloped part of sociological theory, and is much in need of attention. Promising beginnings, however, are to be found.

B. The theory of the dynamic interrelationships of institutions and culture. This is the major point of articulation between sociology and those aspects of anthropology which may be regarded as theoretically distinctive. What may be called the sociology of nonliterate societies cannot be regarded as distinctive of anthropology as a *theoretical* science. It would deal with the selected processes and patterns of the institutionalization of different elements of culture which are not functionally appropriate for institutionalization, such as traditions of science or speculative philosophy or magic. It is within this field that primarily the sociology of knowledge and, for example, the sociology of religion should be placed.

C. The theory of the motivation of institutional behavior. In my opinion, this theory should center about the concept of role, which is the primary name for the focus of the integration of motivation of the individual within the social system. It also would include the sociologically relevant theory of the process of socialization and of what is sometimes called basic personality

or character structure. Another important aspect is the theoretical analysis of the structural generalization of goals in institutional behavior. This whole field is, of course, one of the major points of articulation between the theory of social systems and psychology. One may perhaps speak of this and the next category as in a very broad sense constituting either "psychological sociology" or an essential part of social psychology.

D. The theory of the motivation of deviant behavior and the problem of social control. This branch of sociological theory within the general framework of a theory of motivation and institutional behavior would be particularly concerned with the dynamics of the balance between conforming and deviant behavior. It would analyze the sources of the motivation to deviant behavior in terms of their relations to the social status and role of the individuals concerned, and the strains placed upon them in those situations. Conversely, it would analyze the mechanisms of social control by which deviance is kept at a relatively low level. It assumes both knowledge of institutional structure and of the basic motivation of institutional behavior as a starting point.¹⁰

⁹ Professor Newcomb in his discussion paper questions the distinction between the above psychologically oriented branches of sociological theory. Of course no such classification as the present one can claim finality, and as among the five branches outlined here I should consider this the least important. There is a certain merit of symmetry in the scheme he suggests: A. Theoretical analysis of Institutional Structure itself. B. Relations of Institutions and Culture. C. Relation of Institutions and Motivation.

I should like to suggest, however, that my distinction is useful precisely from the point of view of sociological as distinguished from psychological theory. Psychologically the same fundamental scheme of motivational forces and mechanisms is involved. But in relation to the functioning of the social system there is an important difference of level. One deals with the motivational foundations of behavior in a given social system, the broad structuring of the motivational forces and its psychological determinants. The other deals with the more dynamic and microscopic balance of continuing adjustment processes. They tend to focus on rather different ranges of empirical phenomena, the latter above all on those of "social pathology." A cer-

¹⁰ Cf. the author's introduction to *Max Weber: The Theory of Social and Economic Organization*, Ed. by Talcott Parsons. Oxford University Press, 1947.

E. The final branch of sociological theory according to this conception is the dynamic theory of institutional change. It is most important to realize that this from a theoretical point of view would involve a synthesis of all the other branches of the total theoretical system. It is impossible to understand the dynamics of change without a knowledge of the structural base from which any given process of change starts. It is also impossible to understand it without some knowledge of the possibilities of new definitions of the situation which are available in the cultural tradition. Finally, the motivation of any such change requires explicit analysis of the relevant motivational problems. This is above all true of the starting points of the process of change in terms of what, from the point of view of the initial starting point of a process of change, must be defined as deviant behavior. It must include an analysis of the ways in which deviant behavior becomes socially structured and linked with legitimizing cultural patterns. It is unquestionably the culminating synthetic aspect of the theoretical structure of our science, and high levels of achievement in this aspect must depend on the development of the tools in the other branches

tain relative distinction must be taken account of on whatever classificatory basis.

Newcomb also raised the question of the exact place of social psychology in the present scheme. In particular he suggested a neglect of the biological conditions of social behavior. Whether C and D of this classification should be taken to define the scope of social psychology may be taken to be an open question. It is meant to define a part of sociological theory, and social psychology may well be held to extend farther in the biological direction. I should feel quite definitely that explicit theoretical analysis of the biological determinants of behavior should not be considered part of sociological theory as such.

The greatest care should of course be exercised always to take adequate account of the implications of the facts known about these determinants in all empirical generalizations about social phenomena. But this is quite a different matter from treating a theory of biological determinants as an integral part of sociological theory itself. The issue is essentially one of methodological convenience and elegance—not one of empirical attention or neglect.

with which the theorist of dynamic change must work.¹¹

CONCLUSION

The point of view developed in this paper does not give sociological theory the most ambitious place ever claimed for it. It does, however, give it a key place in the total theory of social systems. It may be argued that institutions constitute as it were the structural "backbone" of social systems. The science which above all focuses on the study of this skeletal backbone is a strategically important part of any large scale study of social phenomena. It can quite definitely be claimed that no other major tradition of social science fulfills this vitally important function in the whole. The nearest competitor would be social anthropology, but the concentration on the cultural point of view in anthropological tradition precludes recognition of its claim in this specific respect. There is a fundamental theoretical difference between the analysis of culture in the anthropological sense and the analysis of institutional structure of the social system. It is, however, of the first importance that the two should be satisfactorily articulated with each other.

A few words should also be said about the relations of sociological theory to two other important bodies of theory in the social field, economic theory and political theory. Economic theory, in my opinion, is concerned with the analysis of the determinants and consequences of certain distinctive dynamic processes of action within the social system, those responsive in certain ways to the stimuli of "prices" or "market

¹¹ It is scarcely necessary to point out that attempting to attain such levels of synthetic analytical generalization without the necessary empirically validated subsidiary particular "theories" of more restricted scope has been the major source of the instability of so many grand theoretical syntheses of the past, such as the great evolutionary schemes. It will require the development of very many of the restricted theories to which Merton refers before more than very restricted accomplishments on this level which are not unduly "speculative" become possible.

conditions." These processes always presuppose an institutional framework within which they take place, and their strategic significance to the social system is a function of the particular character of this framework as well as of other factors. But whatever the role of an "institutional" point of view in many of the empirical branches of economics, in no case does *economic theory* as a distinctive conceptual scheme include a theory of institutions in the sense of this paper.

The case of political theory is different. Certainly one of its main interests has been in a particularly strategic complex of institutions in the above sense, namely those of government. But these have been treated in connection with dominant normative systems of ideas about what the role of government *should* be and of the history of both these ideas and the institutions. As a *theoretically distinctive* conceptual scheme of purely *empirical* social science which is not sociological, economic, anthropological, or psychological, I cannot see that there is such a thing as political theory. Its claim to distinctiveness must be couched in terms of a different level of considerations.

This problem makes it imperative to be quite clear that this paper has been couched in terms of the role of sociological *theory* as a conceptual scheme, not in terms of sociology as a total social science in its relation to others. I do believe that the place of sociology among the social sciences must rest *primarily* on the distinctive role of its *theory*. But, on the one hand, the sociologist working in an empirical field inevitably will and should deal with many things which are not in this theoretical sense distinctively sociological. This is true for instance of the student of population, where institutions are only part of the problem, or of attitudes and public opinion. Correspondingly, many economists, political scientists, or anthropologists will be dealing in part with what is essentially applied sociological theory, since institutions are always to some degree empirically relevant to their interests and sometimes of crucial strategic significance. Indeed *every* important empirical field of

social science is a field of application for the conceptual schemes of *all* the relevant theoretical disciplines. No academic organization of the disciplines can overcome this inherent logical cross- and inter-penetration. In which disciplinary category a given empirical field is predominantly placed is usually mainly a matter of historical accident and pragmatic convenience, not of scientific principle.

In conclusion I recognize fully that what I have presented is not a system of sociological theory. It is rather a program for the development of such a system. It would be possible in the present state of knowledge to spell this out considerably farther in the direction of formulating parts of the system itself. This occasion does not provide time to do this. I hope, however, that many theoretical workers will find this in its broad outline an acceptable approach, and will be able to contribute at many different points of the gradual building up of a well integrated and keenly thought out system of concepts and their logical interrelations which within a reasonable period will earn for sociology a place as one of the more highly developed of the theoretical sciences.

DISCUSSION

Robert K. Merton
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The text for my beginning discussion is provided by Parsons' observation that "the time is ripe for an attempt to deal with theory as a common task of the theoretically interested members of the professional group rather than with 'theories,' the critical discussion of the work of a variety of different people . . . there is every prospect that [various current contributions] should converge in the development of a single conceptual structure." I want to make three points concerning this statement: one expressing partial agreement, the next seeking to extend its implications and the third expressing disagreement.

I can only voice strong agreement with the view that the day of rival schools of sociological theory, each purveying its system of doctrine in the marketplace of sociological opinion or engaging in open academic warfare with its enemies, has come to a well-deserved close. As I

shall suggest later, these controversies were in large part a product of the urge to create total systems of sociological thought rather than to create small families of empirically verified theorems.

An implication of Mr. Parsons' remark deserves fuller comment. The attractive confusion of sociological theory with the *history* of sociological thought should long since have been dispelled. This is not to deny the great value of steeping oneself in the history of sociological thought. It is only to deny that the history of theory and currently applicable theory are one and the same.

Schools of medicine do not confuse the history of medicine with current theory, nor do departments of biology identify the history of biology with the viable theory now employed in guiding and interpreting research. Once said, this seems so obvious as to be embarrassing. Yet the extraordinary fact is that in sociology, this distinction between the history of theory and current viable theory has not caught hold—at least, if we take existing curricula and publications as evidence. There are many courses in the history of sociological thought—who said what by way of speculation or theory?—but relatively few courses on the systematics of those theories with which sociologists *presently* operate.

The history and the systematics of sociological theory should of course both be our concern, but this is no reason for merging and confusing them. The theories of a Comte or a Spencer, of a Hobhouse or a Ratzel are chiefly of historical interest—little of what they wrote remains pertinent today. They were, perhaps, grand achievements for their day, but that day is not ours. They testify to the large merits of talented men, but they do not provide guidelines to the present analysis of sociological problems.

Current systematic theory represents the selective accumulation of those parts of earlier theory which have survived the test of decades of research. The history of theory includes the large conceptions which were dissipated when confronted with rigorous tests; it includes also the false starts, the archaic doctrines and the plain errors of the past. Though acquaintance with all this is a necessary part of the sociologist's theoretic equipment, it is no substitute for the current systematics of theory. The identification of the two must be replaced by their allocation to distinct and only loosely related spheres.

To this point, I agree wholly with what I assume to be the implications of Mr. Parsons' statement. But when he suggests that our chief task is to deal with "theory" rather than with "theories," I must take strong exception.

The fact is that the term "sociological theory," just as would be the case with the terms "physical theory" or "medical theory," is often misleading. It suggests a tighter integration of diverse working theories than ordinarily obtains in any of these disciplines. Let me try to make clear what is here implied. Of course, every discipline has a strain toward logical and empirical consistency. Of course, the temporary co-existence of logically incompatible theories sets up a tension, resolved only when one or another of the theories is abandoned or so revised as to eliminate the inconsistency. Of course, also, every discipline has basic concepts, postulates and theorems which are the common resources of all theorists, irrespective of the special range of problems with which they deal.

But review the experience of these disciplines more carefully. Note how seldom they deal with "physical theory" or with "chemical theory" and how typically they deal with the theory of *specific types* of phenomena: for example, with the kinetic theory of gases or the wave theory of light or the lattice theory of crystal structure. Of course, distinct theories often involve partly overlapping concepts and postulates. But the significant fact is that the progress of these disciplines consists in working out a large number of theories specific to certain types of phenomena and in exploring their mutual relations, and not in centering attention on "theory" as such.

Now without implying that the social sciences must uncritically follow the practices of their elder siblings among the sciences, I believe that this provides an object-lesson. In the early stages of a fledgling discipline, its exponents typically make extravagant claims to having evolved total systems of theory, adequate to the entire range of problems encompassed by the discipline. But complete sociological systems, as in their day complete systems of medical theory or of chemical theory, must give way to less imposing but more adequate sets of limited theories. We cannot expect any individual to create an architectonic system of theory which provides a complete *vade-mecum* to the solution of sociological problems. Science, even sociological science, just isn't that simple.

Sociology will advance in the degree that the

major concern is with developing theories adequate to limited ranges of phenomena and it will be hampered if attention is centered on theory in the large. I am confident that this is not in basic disagreement with Mr. Parsons; that it is a difference in emphasis rather than substance; indeed, later passages in his paper suggest as much. But I think it important to supply just that emphasis. I believe that our major task today is to develop special theories applicable to limited ranges of data—theories, for example, of class dynamics, of conflicting group pressures, of the flow of power and interpersonal influence in communities—rather than to seek here and now the “single” conceptual structure adequate to derive all these and other theories. To say that both are needed is to be correct and banal; the problem is one of allocating our resources. I am suggesting that the road to an effective conceptual scheme will be the more effectively built through work on special theories, and that it will remain a largely unfulfilled plan, if one seeks to build it directly at this time.

That this emphasis may be needed can be seen from a review of books on sociological theory. Note how few, how scattered and, it must be said, how unimpressive the specific sociological theories which are derived from a master conceptual scheme. The basic theory, so-called, runs so far ahead of confirmed special theories as to remain a program rather than a consolidation of these theories. This is no dirge. As Mr. Parsons has noted, much progress has lately been made. The gradual convergence of some blocks of theory in social psychology, social anthropology and sociology promises a new era of large theoretic gains. Yet having said this, it must be admitted that a large part of what is now called “sociological theory” consists of general orientations toward data, suggesting types of variables which need somehow to be taken into account, rather than clear, verifiable statements of relationships between specified variables.¹ We have many concepts but few confirmed theories; many points of view, but few theorems; many “approaches” but few conclusions. Perhaps a shift in emphasis would be all to the good.

Sociological theory must advance on these inter-connected planes: through special theories

adequate to limited ranges of social data and through the evolution of a conceptual scheme adequate to consolidate groups of special theories.

To concentrate solely on special theories is to run the risk of emerging with *ad hoc*, unrelated speculations consistent with a limited range of observations and inconsistent among themselves.

To concentrate solely on the master conceptual scheme for deriving all sociological theory is to run the risk of producing twentieth-century equivalents of the large philosophical systems of the past, with all their suggestiveness, all their architectonic splendor and all their scientific sterility.

Men allocate their scant resources somehow, whether they know it or not, and this allocation reflects their workaday policies. This holds as much for the men concerned with the production of sociological theory as for men concerned with the production of plumbing supplies. These comments elicited by Mr. Parsons' paper are intended to bring out one such policy-decision faced by the men who practice sociological theory. Which shall have the greater share of our immediate energies and resources: the search for confirmed special theories or the search for all-inclusive conceptual schemes? I believe, and beliefs are notoriously subject to error, that for some time to come, it is the special theories which hold the largest promise, provided that, underlying this modest search for social uniformities, there is a pervasive concern with consolidating the special theories into a more general set of mutually consistent propositions.

To these views on a policy for sociological theorizing, I can only append, but not fully discuss, five further points in Mr. Parsons' paper which call for elaboration. These points I believe to be central to sociology since the stance taken toward each of them will profoundly affect the nature of the resulting theories.

1. When Mr. Parsons notes that the “social situation” must be analyzed with respect to “the various types of significance of situational facts to the actor,” there is need for further strict clarification. Does this mean that the sociologist takes into account *only* those aspects of the objective situation to which the acting individual is oriented (cognitively, affectively, or through goal-definitions)? Does it imply that observable aspects of the situation of which the acting individual is *wholly unaware* are at once

¹ This observation has been somewhat amplified in my paper on “Sociological Theory,” *American Journal of Sociology*, 1945, 50, 462-73.

eliminated from the realm of facts pertinent for the sociologist? If so, one must register dissent. Does the sociologist seeking to interpret, for example, the continuing optimism of small businessmen in the face of repeated failure wholly neglect the role of governmental decisions in the total situation, even though individual businessmen may be entirely unaware of these decisions or of their impact on their own life-chances? I should think not, and I know that Mr. Parsons thinks not. But it is all the more important to clarify this formulation, else one might suppose that he advocates a basically idealistic or subjectivistic approach to sociological theories, in which only those aspects of the situation somehow taken into account by individuals are considered pertinent to the sociological analysis.

2. When he speaks of understanding the action of an individual only if we take into account his "attempts to know it *cognitively*, in terms of the *goals* he is striving to achieve and in terms of his *affective* attitudes," Parsons, as he doubtless recognizes, is resurrecting the ancient categories of cognition, volition and affection. Now great age, of conceptions as of men, is no sufficient warrant for accepting—or for rejecting—their authority. But one should consider that this is a repeatedly abandoned and re-adopted classification of behavioral aspects involving in each sphere a large number of specific behavior-elements. One should be sensitive to the possibility that the elements in each of these spheres may be so heterogeneous as to put such general categories into serious question. Premature categorization may be almost as dangerous as failure to categorize at all.

3. I should agree at once that the concept of "functional needs" (or prerequisites) is basic to any functional analysis of social systems. I should urge, however, that this is at present one of the cloudiest and least articulated concepts in the entire panorama of functional conceptions. For reasons of space, Mr. Parsons confined himself to a statement of three broad functional needs of any social system: prerequisites for biological survival of a "sufficient" proportion of its members; arrangements adequate to permit a necessary minimum of coordination of activities for members of the society and for the social system as a whole; and arrangements for motivating people to carry out "essential" social roles.

Since these are general categories of functional needs, they are formal in character. And this is

precisely where present notions of functional needs run into serious difficulties. Consider the following drawn from a longer list of difficulties entailed by such formal concepts of functional needs:

(a) The notion of "need" is, as Hull has shown an intervening variable. To be useful as a systematic concept, it must be anchored *both* to observable antecedent conditions *and* to observable consequences. Otherwise, the concept is in danger of being tautological or *ex post facto* in character. Yet these requirements for the effective use of intervening variables are seldom satisfied by currently postulated needs of social systems.

(b) Mr. Parsons puts explicit emphasis on functional requisites for the *maintenance* or *survival* of any given social system. This is in accord with prevailing tendencies in functional sociology. But this does not adequately direct the observer's attention to the *kind* of social system which is to survive. The biological analogy, often useful, is here misleading. For what of the "functional needs" for a basically changed, though surviving, social system in which the positions of the social structure are significantly altered, even though the human beings implicated in the social system remain largely intact? All intent aside, emphasis on the need for survival tends to focus attention of observers on statics rather than on change and to divert attention from the functional needs for determinate types of change.

In the same way, the *formal* problem of maintaining *some* social order does not direct us to the *range of social orders* possible among the same population of interrelated individuals. There may be an "order" in which the bottom rail comes to be on top or an "order" in which the top rail remains where it was. Both represent "social orders" but with very different consequences for diverse members of the society.

Now Mr. Parsons would be among the first to recognize all this; indeed, this is also implied later in his discussion. But again we must note that the difference between explicit emphasis and implicit acceptance is often great enough to affect the course of inquiry. For it is my strong impression, gained from systematic examination of many functional analyses, that the concern of functional sociologists and anthropologists with problems of "order" and with the "maintenance" of social systems has focused their scientific attention on the study of processes whereby a given system is preserved large-

ly intact rather than on the processes utilizable for determinate basic changes in social structure.

(c) Over and above all this, I can only mention, in a manner in which brevity may be mistaken for dogmatic assertion, that my examination of functional studies shows that a clear minimum inventory of "functional needs" of social systems has not yet been developed. Malinowski's brave failure is but a single though impressive witness to the more general deficiency. When one departs from the general, formal plane of "order," "survival," "motivation" and "socialization" to the plane of specific needs demonstrably identified in given social systems, agreement gives way to widely dispersed opinion. There is yet to be published a demonstrably useful set of specific functional prerequisites at all paralleling that of the physiologist, typically taken by functional sociologists as a prototype. In passing, I suggest that this shortcoming arises from a familiar but nonetheless basic methodological difficulty: the ready recourse of physiologists to experiment designed to test the usefulness of their intervening variables of specific functional needs is not adequately matched by sociologists' recourse to comparative social and cultural data or to quasi-experimental inquiry with small groups. It is my impression that *methodological* limitations largely account for the present inadequacy of *substantive* theories of functional needs.

(d) Much the same can be said of the formally described need for motivating people to perform "the essential social roles" in a society. The criteria of the "essential" are of course heavily dependent on the social system as it exists at a given time. In actual practice, functional sociologists devote little attention to alternative roles "essential" to the modification of a social system in determinate directions.

4. In his brief presentation, Mr. Parsons has not explicitly treated several concepts which point away from this emphasis on the social system as given. Chief among these are the concepts of social dysfunction, manifest and latent functions, functional substitutes and equivalents, the diverse social units subserved by a given function, *etc.* These and kindred concepts could not, of course, be systematically treated in brief compass (either in Mr. Parsons' paper or in this discussion of his paper). But they happen to be the very concepts which are necessary to avoid a tacit bias otherwise entailed by functional sociology. Unless these receive due emphasis in however brief a statement

of this theoretic point of view, functionalists will continue to turn their attention upon an unfortunately limited and, I believe, misleadingly simplified set of scientific problems.

5. Finally, Mr. Parsons puts forward an admirable brief statement of the large place reserved in contemporary sociological theory for inquiry into the motivation of deviant behavior and problems of social control. I need only add that this position requires great clarification of the concepts of "deviant behavior" and of "social institutions." For once we speak of "deviation from institutional requirements," we must recognize, with Parsons, that such deviation may also be viewed as a new pattern of behavior, possibly emerging among subgroups at odds with *those* "institutional patterns" which are supported by certain power groups and by certain legal controls. Unless the theory of social institutions includes systematic consideration of the specific groups which support given "institutions," it overlooks the important role of sheer power in society. To speak of the "legitimation of power" is to use an elliptical (and consequently, often misleading) phrase. For power may be legitimized for *some* without being legitimized for *all* groups in a society. It may, therefore, be misleading to describe non-conformity with particular "social institutions" as "deviant behavior."

DISCUSSION

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System-making is not only necessary; it is also fun. In this respect I suppose we are all very much like a child watching grown-ups fit together the pieces of track and switches for an electric train. No matter how well the grown-up does it, the youngster wants to try it for himself. With similar *disrespect* for wisdom and experience, I shall re-arrange some of Professor Parsons's pieces—without, however, trying to create new ones. My re-arrangements, moreover, will be restricted to the periphery of the system. The heart of it shall leave untouched—i.e., his contention that institutions provide the theoretical focus of sociological science. A correctly placed heart should not be tampered with.

My major proposed re-arrangement has to do with two of his "divisions" of sociological theory: "the theory of motivation of institutional behavior" and "the theory of motivation of deviant behavior and the problem of social

control." The distinction of these two sorts of motivation seems to me of the kind which Kurt Lewin would call phenotypical rather than genotypical. In terms of social psychological theory (which, I agree, should center about the concept of role) there seems to me no important difference between motivated roles which are conforming and those which are not.

From a psychological point of view, the process of learning to respond in certain ways to other people is the same whether the end result is a conforming or a deviant role. In either case the process is one of goal-directed behavior involving perception, performance, thought and affect. The goal toward which behavior is directed while institutionally prescribed roles are being acquired is not necessarily "to acquire the prescribed role." Roles are also acquired while behavior is directed toward food, fun, relaxation, or almost anything else. From the individual's own point of view he is simply acting in ways available to him in order to get what he wants to get. In Linton's pat phrase, "He takes the bait of immediate personal satisfaction and is caught upon the hook of socialization." Taking the bait, however, often results in socialization of a deviant sort. There are not two kinds of theory here, but one.

Professor Parsons brackets "the problem of social control" with that of "the theory of the motivation of deviant behavior" for the very good reason that it is the latter kind of behavior which social controls are designed to prevent or to correct. This particular piece of track seems to me, however, to belong in his first "division," dealing with "the structural differentiation and integration of institutional patterns." He quite properly included in this "division" the "theoretical analysis of the relations of structure and structural variation to the functional needs of social systems." What is generally known as "control" is surely one of those functional needs.

My proposed re-arrangement is therefore that the theory of motivation of institutional and of deviant behavior be considered as a single "division," and that "the problem of social control" be included in his first. Having eliminated his fourth "division," I have a similar fate to propose for his fifth, though for quite different reasons.

It seems to me that what Professor Parsons basically wants can be stated under three rather clearly defined headings. He wants, first, a system of social theory which is solidly rooted in

the study of institutions. Secondly, he wants this system to be so devised that it can be articulated effectively with other systems on either side of it, so to speak—i.e., with motivation theory and with culture theory. Thirdly, he wants the entire system to be so formulated that changes through time can be studied. I am sure he would be the first to agree that a "dynamic theory of institutional change" must be one which is capable of relating institutional change to changes in individual motivation on the one hand, and to cultural changes on the other. This seems to me to be equivalent simply to adding another dimension to the system, rather than pointing to the need for another "division." What his system boils down to thus seems to me to be something like this: a central core of theory organized about the structural-functional relationships of institutions; one horizontally collateral body of theory which is articulated both with institutional and with psychological theory; and another horizontally collateral body of theory articulated with the theory both of institutions and of culture. Each of these bodies of theory must be so formulated that they can be related to each other in the vertical as well as in the horizontal dimension—i.e., through changes in time as well as at any given moment. This amounts to a three-divisional, two-dimensional body of theory instead of a five-divisional one of which one division deals primarily with the time dimension.

I should like to focus the remainder of my remarks around the field of social psychology, that one of the horizontally collateral bodies of theory which I know best. These remarks are offered not as a critique, nor even as a rearrangement, of Professor Parsons's proposals, for I find myself in virtually complete agreement with them. My comments, rather, are an extension of his.

The history of social psychology may be likened to the digging of a tunnel. Sociologists did the first digging, starting from their own side of the mountain. Their information as to what lay on the psychological side of the mountain was necessarily limited and was mainly applied by way of speculation and analogy. The psychologists, who started burrowing somewhat later, had a vague sense of direction, but no map of the terrain where they needed to emerge. These two tunnels have been a-building for more than a half-century now, and they are still nowhere near meeting. They are not even going toward each other. The two teams of engineers

scarcely bother to read each others' maps, though they have free access to them.

As a crude method of documenting this statement I have examined the index entries in two of the most recent and widely used texts in social psychology, one by a psychologist (Bird) and one jointly by a sociologist and a psychologist (LaPiere and Farnsworth). The first has 233 major (i.e., non-indented) entries, the second 130. Of these, exactly 34 are equivalent, or 15% and 26%, respectively, of the two volumes. Perhaps you will be interested in knowing some of the omissions in each of them. The following terms do not appear in Bird's index (nor their equivalents, as far as I can discover): assimilation, caste, class, communication, community, contagion, culture trait, ethnocentrism, family, fashion, gang, in-group, institution, interaction, isolation, mob, mobility, mores, person, primary group, public, role, self, socialization, society. I do not find the following in LaPiere and Farnsworth's index: aggression, attitude measurement (except in the appendix), conditioned response, conservatism, control group, drive, frustration, perception, role (except as applied to occupations), social facilitation.

For this condition, which I believe you will agree is little short of scandalous, I have a diagnosis to offer and a remedy to propose. We have two types of social psychologies rather than one—not to mention the various brands of each type—for two major reasons. First, both sociologists and psychologists have thought of the field as a colony of their own. I agree completely with Professor Parsons when he says that psychological assumptions do not "lie outside the system," but are "an indispensable part of the system itself." But the system of which he is speaking is not just a sociological system, but an entire system of social theory. He is asking, just as I am, for real articulation. But a social psychology which is really articulated with *both* psychology and sociology can be a branch of neither.

The one central fact of social psychological life is that biological organisms become profoundly modified by their membership in groups and societies. Social psychological theory as a mere branch of sociological theory has always tended to ignore the bio-psychological conditions that limit and determine the ways in which socialization can take place. As a mere branch of psychological theory, it has always tended to ignore the actual structuring of the "social field" which limits and determines the ways in

which bio-psychological changes can take place. Either of these omissions I believe to be fatal for any adequate theory, and yet I believe both will continue as long as social psychology is thought of as a mere branch of the other fields. The kind of theoretical articulation which Professor Parsons wants to see can be achieved, I believe, not just by attempts to articulate, but only if and as social psychologists develop their own body of theory which, necessarily but incidentally, is articulated both with sociology and with psychology. This is not the time to outline such a theory, but its essential ingredients seem to be already available. Its bare minima would include a theory of motivation which takes account of the facts of physiological drive, perception and affect; which links motivation to social roles, both institutional and deviant; and which links social roles to the multiple group structuring of society. Its concepts must not only apply *both* to individual behavior *and* to the social field in which it occurs; in particular, they must also point to those intervening variables by which the individual organism relates its behavior to the social field. Without such intervening variables we have no theory of the interdependence of organism and social field. Such a theory, based upon such concepts, "belongs" neither to sociology nor to psychology, but it will be in a position to contribute to both.

The second reason for the scandalous condition which I have described is that too few social psychologists have made more than casual use of the "genuinely operational concepts" which Professor Parsons demands. "The ideal," he says, "is to have theoretical categories of such a character that the empirical values of the variables concerned are the immediate products of our observational procedures." Amen. Our failure here is closely related to the sins of omission which I have already catalogued. Theories phrased in terms of operational concepts are testable, and two bodies of social psychological theory could not long continue to exist in quasi-independence if both were stated in testable terms. We have long excused ourselves on grounds that the phenomena with which we deal are "complex." I don't think much of the excuse. In some part, at least, it serves as a rationalization for existing preferences and even for laziness. If we really believe, for example, that group memberships determine roles which determine motives which in turn determine vast segments of behavior, why don't we put our belief to the test instead of relying upon anecdotal

dotal evidence? There is not a single term in the preceding sentence which cannot be defined in terms of observational procedures, nor a single independent variable which cannot be systematically varied, either experimentally or by field procedures.

It seems to me only fair to add that we sociologists are even more delinquent in this respect than our psychological colleagues. Both of the following statements are over-simplified, but there is much truth in them. Sociologists have invented or nurtured a large proportion of the concepts which are central to social psychology—e.g., attitude, self, role. Those who have put them to the empirical test have more frequently been psychologically trained. If, as I suspect, the frontiers of social psychology today are being pushed back more boldly by psychologists than by sociologists, it is because the former have learned somewhat more successfully to wield as tools concepts which are operational. It is not, emphatically not, because the kinds of variables with which they characteristically deal are more important. I think there is actual danger that, if this trend continues, sociologists may find themselves left out of the field which they did so much to create. The

loss to social psychological theory would be immense, or even fatal.

It seems to me that while these remarks constitute an extension of Professor Parsons's paper, it is an extension which is for the most part inherent in several of his most basic points. "The theoretical system which is basic to sociology," he says, "must be broader than the science of sociology itself. It must be a theory of social systems. . . . We must somehow work out a theoretical scheme which articulates our field with others which are equally part of the same broader fundamental system." I have tried to point out that this broader fundamental system must include a social psychological theory which, on the one hand, is not dependent upon sociological theory, any more than the latter is dependent upon culture theory. Neither, on the other hand, must it be independent of sociological theory, any more than the latter is independent of culture theory. They must be interdependent in the sense that each stands on its own feet, but each borrows from and lends to the other. This kind of productive interdependence is most likely to come about if both are rooted in the solid ground of testable hypotheses.

CONCERNING ETHNIC RESEARCH*

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I TAKE as my assignment to open up for discussion by the Panel some of the central problems encountered by investigators in this field.

You will note that I use the word *ethnic* in its omnibus sense so that we may consider studies of all groups which are set off from the rest of the population on the basis of racial criteria, religious identity, cultural attributes, or national or ancestral backgrounds. For example, on the Pacific Coast, in addition to Negroes, Orientals, Jews, and several nationality groups, one would have to include the Okies even though they are old American, racially invisible, and by cultural criteria only weakly differentiated. In an area of investigation so broadly de-

fined one would face up to such important considerations as acculturation, in-group formation, which lie at the basis of race relations so-called but which too often are not entertained because they do not directly and immediately involve ethnic interaction.

Because sociology is still known as a textbook science it may be symptomatic that no general work adequately covering the field of ethnic relations has been published since Young's *American Minority Peoples*¹ in 1932. I make an exception of the valuable collection of essays under the title *Race Relations and the Race Problem*² which does not presume to be a textbook.

¹ Donald R. Young, *American Minority Peoples*. N.Y.: Harper and Bros., 1932.

² Edgar T. Thompson (ed.), *Race Relations and the Race Problem*. Durham: Duke University Press, 1939.

* Paper read before annual meeting of the American Sociological Society, New York City, December 28-30, 1947.

A few more remarks may be made in introduction. Between the 1934 Meeting, which you will recall was devoted to Racial and Cultural Contacts, and this one, there has been no section devoted to ethnic relations. Similarly the Census of Research has made no provision for separately reporting research on race and culture, although the classification was introduced this year. Both in the Meetings and in the Census, papers in this category have been randomly reported, for example, in community and ecology, social psychology, and, when the classification was in vogue, social biology. The Kennedys³ in their article in the *Review* in 1942 found that nearly 4% of 5500 courses in sociology were devoted to ethnic studies. The field ranked tenth among the various subject matter areas, and is an important element in most courses called social problems. In a perusal of the *Review*, I find that nearly 10% of the articles in the last 12 years pertain in one way or another to ethnic problems, nearly half of which are devoted to the Negro. Because organizational devices are unduly important in our society, one hopes that the field will have institutional support and recognition of its very real existence.

It is safe to say that there is no subject matter of sociology more important to the general public and none either within or outside academic circles which has been affected by a greater sense of urgency. One of the principal weaknesses in the field is that the amount of emphasis is proportional to the amount of tension in inter-ethnic relations. Violence or the threat of violence invites investigation far out of proportion to the social space that it occupies. Studies of tension as such and of race relations, particularly in their symbolic manifestations as in attitude and opinion studies, loom very large. The detailed and exact analysis of the differentiation of ethnic groups, which is a precondition for properly designed research, gets relatively scant attention. The distribu-

tion and organization of power gets even less attention, except by inference from economic status.

Because the problem of the Negro looms so large in American life, the study of race relations since the end of mass immigration from Europe has tended to be the study of the American Negro. The delimitation of *An American Dilemma*,⁴ for whatever reason, to this aspect of the American ethnic complex was in my opinion regrettable. A more inclusive interpretation of *An American Dilemma* would have encouraged comparisons, would have favored cross-sectional over historical techniques of investigation, and would have increased the orientation value of a work which will be taken as definitive for a long time.

Much less has been written about groups other than the Negro, although as a by-product of the war we suddenly found ourselves with a considerable amount of information about Americans of Japanese ancestry. Probably never before have we had such a large body of knowledge about such a compact group, but it is too early to decide how adequate the data are and how well they may be utilized for purposes of general research. A great part of the information was compiled with the intention of answering specific and narrow administrative questions and will therefore be of slight utility. At least, however, we have populational data which enable us to establish statistical controls and to set up research projects on Japanese-Americans with more precision than for any other American ethnic group. The reasons for this distinctively advantageous situation are worth mentioning. Census findings, which ordinarily would have been prohibitively expensive, became available through the Army⁵ and were augmented by the records of the War Relocation Authority. The latter leave something to be desired for research purposes, but would be enormously improved if the master file,

³ Raymond and Ruby J. R. Kennedy, "Sociology in American Colleges," *American Sociological Review*, VII, No. 5 (October 1942), 661-675.

⁴ Gunnar Myrdal, *An American Dilemma*. New York: Harper and Bros., 1944.

⁵ Western Defense Command, *Statistical Bulletins*.

which I understand is now in the hands of the Justice Department, were released for scholarly purposes. This matter, however, belongs in the province of the Committee on Government Records and Research of the Social Science Research Council. Also as an outgrowth of the war a number of private research projects on Americans of Japanese ancestry were initiated, both in the continental United States and Hawaii. Research projects on evacuation and resettlement promise to document this curious page in American history. Above the level of pure documentation, material is afforded for a thorough analysis of the population and the basis is laid for a provocative series of comparative locality studies. Whether or not this opportunity will be seized depends on our ability to organize research on criteria other than the size of the group. In any event, work in Hawaii and possibly in Brazil affords us an opportunity for comparative analysis, and one may hope that the research will be extended to Japan.

There have been substantial programs of ethnic research in other areas, notably Brazil, Hawaii, and South Africa; but continental American sociologists appear reluctant to go outside their own area for comparative data with which to illuminate the propositions they are examining in the local scene. The consequence is a parochialism and a tendency to erect hypotheses on an overly particularized foundation. Important problems would be posed better and generalizations would be more fully established if a given question were answerable from several world or domestic areas or from the points of view of different groups in the same area.

In touching on the question of comparisons, one must mention our bizarre state of ignorance about our Mexican population. In part this is an artifact of the operations of the Bureau of the Census and the declaration that Mexicans are "whites" for the purposes of the 1940 census. The 1930 census, the only one which classified Mexicans among "other races," is suspended in time and we are left without one of the pre-

conditions for research on our third most numerous ethnic group. This querulous note is injected with the hope that in those areas where persons of Mexican ancestry are concentrated the Census will employ some device such as linguistic questions, so that research on this very important population will be facilitated.

The most orderly kind of research on ethnic groups, it seems to me, would be built upon a solid empirical base of knowledge about the characteristics of the population, their manifestations of solidarity and structural integration on the one hand, and their differentiation and inter-group relations on the other. Such knowledge might be initially, perhaps even *should* be, on a low level of conceptualization, but so fully presented that the data could be rephrased from several different methodological points of view, much as one can manipulate the data of a factor matrix. This plea for a solid empirical foundation does not mean that I am unmindful of the reciprocal danger of purposeless fact-gathering and theoretical sterility. The findings of a locality study should be available for analysis from the points of view of ecology, formal stratigraphy, power distribution, inter-areal comparison, acculturation, demography, and social structure and processes. Because of preoccupation with methodological axe grinding, some of the more pretentious reports have been narrowly conceived and limited in applicability. This has stigmatized the locality study and diverted research workers to other approaches.

But making the fullest allowances for methodological experimentation and focusing upon the Negro about whom so much has been written, we find ourselves with a disconcertingly small stock of reasonably comprehensive community or urban descriptions. At one end, 1899, is W. E. B. DuBois' study of the *Philadelphia Negro*.⁶ At the other, 1945, is Drake and Cayton's *Black Metropolis*.⁷ Undoubtedly *Black*

⁶ W. E. B. DuBois, *The Philadelphia Negro*. Philadelphia: University of Pennsylvania Press, 1899.

⁷ St. Clair Drake and Horace Cayton, *Black Metropolis*. N.Y.: Harcourt, Brace and Co., 1945.

Metropolis is as good as it is because *The Negro in Chicago*⁸ together with the Chicago ecological experiment supplied data which satisfied the conditions of empirical comprehensiveness and adaptability. For half a century, however, we have managed to ignore the recommendation of DuBois: "If, for instance, Boston in the East, Chicago and perhaps Kansas City in the West, and Atlanta, New Orleans and Galveston in the South, were studied in a similar way, we should have a trustworthy picture of Negro city life."⁹ This was surely a sensible and foresighted program for Negro studies and one that deserved more consideration from American Social Science than it received. One can not do better now than to reiterate DuBois' ideas, slightly redesigned. For one thing, as a semi-centennial of the *Philadelphia Negro* we should have a reappraisal of the situation there, in so far as possible contrived to exploit DuBois' work as a baseline. Then we should have a series of studies of those places which represent way-stations on the movements to one of the main metropolitan centers, preferably Chicago, thus adding another dimension to that admirable document, *Black Metropolis*.

Until the empirical foundations are laid the worker will be hampered in designing experiments, however intelligently conceived his approach. Williams,¹⁰ for example, in one of his numerous provocative suggestions for research, manages to get the cart squarely before the horse. He suggests comparing some recreational programs in several areas of conflict, one a Mexican-native situation in Los Angeles. Despite a variety of hedges, the research could not be controlled in our present state of ignorance. Among the Mexican population in the Metropolitan area, localities differ in such significant variables as acculturation, regional origin, time of mi-

gration, and stability, any one of which might be crucial. If the kind of research proposed by Williams is performed, it will be done wastefully unless based upon a thorough differentiation of the populations under analysis.

We are little, if any, better off in our synoptic descriptions of smaller places. There immediately come to mind Powdermaker's *After Freedom*¹¹ and Dollard's *Caste and Class in a Southern Town*,¹² and *Deep South* by Davis and the Gardners.¹³ Although fragmentary and obscure items exist, one cannot be sanguine about the possibilities for comparative analysis until there are more studies primarily descriptive in nature. I do not want to imply that the descriptive document must be a substantial monograph if it is to be useful. Shorter reports, such as the recent paper by McCormick and Hornseth, "The Negro in Madison, Wisconsin,"¹⁴ can be executed economically and lend themselves readily to comparative treatment. What is needed is some agreement on minimum content, and selective care that all the communities do not turn out to have colleges.

The descriptive studies about Jews, with the exception of Wirth's salient work, *The Ghetto*,¹⁵ and more specialized reports such as Koenig's¹⁶ are even more limited. There is a considerable amount of documentation of Jewish communities from the standpoint of the more highly organized and integrated features, but they tend to give an oblique and astigmatic view of Jewish culture and society. Limited institutional studies may,

¹¹ Hortense Powdermaker, *After Freedom*. N.Y.: Viking Press, 1939.

¹² John Dollard, *Caste and Class in a Southern Town*. New Haven: Yale University Press, 1937.

¹³ Allison Davis, Burleigh B. Gardner and Mary R. Gardner, *Deep South*. Chicago: University of Chicago Press, 1941.

¹⁴ *American Sociological Review*, XII, No. 5 (October 1947), 519-525.

¹⁵ Louis Wirth, *The Ghetto*. Chicago: University of Chicago Press, 1928.

¹⁶ Samuel Koenig, "The Socioeconomic Structure of an American Jewish Community," pp. 200-242 in *Jews in a Gentile World*. I. Graebner and Britt (eds.), New York: Macmillan, 1942.

⁸ Chicago Commission on Race Relations, Charles S. Johnson (ed.), *The Negro in Chicago*. Chicago: University of Chicago Press, 1922.

⁹ DuBois, *Op. cit.*, p. 12.

¹⁰ Robin M. Williams, Jr., *The Reduction of Intergroup Tensions*. Social Science Research Council Bulletin No. 57, New York, 1947, p. 100.

of course, justify themselves on pragmatic grounds.

An emphasis on empirical studies has the great advantage at this time of being founded in well-established research procedures. Our science is technologically prepared to embark on a full scale ethnographic analysis of American minority groups. With the exception of the Mexican, field reconnaissance has been done. There was substantial exploration of Negro communities in connection with the Myrdal program and a good deal could be mined out of unpublished documents. In addition descriptions of discrete institutional forms, which comprise the bulk of so-called community studies and which rarely can be built into a synoptic picture of ethnic life, could contribute in a variety of ways.

The program to which I am pointing would have to be planned in a grand manner with more funds than are usually available at one time for sociological research. Doubts have been expressed about the capacities of the sociologist to make large research expenditures wisely. These are doubts which I am inclined to share if the research design be unduly particularized. On the other hand, we must explore all possible techniques, and our journals may serve us well even if they remain the ditty bags of some rather intelligent men. If and when a large series of studies is initiated, the first criteria—but not the final objectives—should be empirical adequacy and technical competence. The program should provide the basis for an areal typology, the comparison of ethnic groups in a given area, and comparative analyses of populations in different areas. Such empirical findings need not lie fallow, but they must not be so contrived that they are only relevant to a very limited set of thin and verbal constructs.

If one is primarily interested in the analysis of inter-ethnic tensions, he might do better whenever empirical background is lacking to deal with situations less complicated than race relations. I assume that inter-ethnic antagonisms are not unique but have something in common with other situations involving sharp cleavages and strong loyal-

ties, such as the situational rivalries attending athletic contests. These involve overt expression of group membership and provide for well canalized manifestations of resentment. A good deal of such behavior occurs within a closed system and there are relatively few ways in which a contest can resolve itself. Observations could be freely made on the full range of aggressive behavior and related to variables like success or failure, the anticipation of success or failure, and the traditions of one rivalry as compared with another. Under some circumstances direct experimentation could be performed through rumor and press reports. At least the *ex post facto* experiment would be possible. The findings of this sort of research could be used in designing more complex research on more serious tensions.

In contrast with the relative dearth of locality studies, there is a wealth of literature on attitudes and opinions. Under the combined operations of social psychologists, sociologists, and others, substantial progress has been made in scale construction and scoring. We have come a long way from the meaningless summaries of discrete all-none responses made by an inadequately described population to a set of possibly related but indeterminate propositions. Most of the findings, however, are insignificant for our purposes both descriptively and analytically. On the descriptive level even crude versions of the Bogardus social-distance scale might have yielded valuable results in the aggregate had the population been stratified by such variables as age, sex, race, religion, class, and the like, and differential responses been fully reported. Thus we should have a growing body of data available for recombination and reinterpretation. This has not been done because the assumed homogeneity of the college population is gratefully taken as adequate reason for not executing a variety of tabulations, which, under the usual conditions of inadequate equipment, are exceedingly burdensome. A more dangerous assumption is often implied, namely that the college population is representative of the total population. The historical modifiability of attitudes, as has

been demonstrated by Farnsworth,¹⁷ results in an unstable reliability for standardized scales. Underlying these serious defects is the fact that many of the studies have not been sociologically pertinent, but rather have been directed at techniques of scale construction and scoring or a discovery of the nature of attitudes *qua* attitudes. Some exception must be made of studies of changes in attitudes, which, however, are usually not completely reported.

An example of a conceptually mature attitude study is Campbell's paper on "The Differential Ordering of Minority Groups on Five Aspects of Prejudice."¹⁸ With considerable insight he has extracted from the social distance scale a series of components which are highly meaningful and readily elicit situational hypotheses. Another case in point is Hatt's provocative article "Class and Ethnic Attitudes,"¹⁹ which uses factor analysis to demonstrate a relationship between patterns of attitudes including prejudice and such variables as class membership and age. Not the least virtue of the latter report is a full statistical presentation of the data.

Although the opinionnaire is statistically a less sophisticated and refined instrument, it is probably more valuable than the attitude scale in the documentation of topical events. For sociological purposes the opinionnaire may be administered intensively in areas defined by ecological criteria, and the findings may be related to the known characteristics of the locality. In my judgment it is unfortunate that a great deal more has not been done with such techniques in the field of ethnic relations.

The concept of relations which is one of the dominant themes in attitude-opinion research has been generally interpreted in a very limited fashion. For instance, attitude studies have with rare exceptions been uni-

directional, the bearer of the attitudes being the presumptively undifferentiated majority and the object of the attitudes one or more minorities. This curious defect violates two salient and valid postulates of sociology: a) that response systems should be understood as interactions, which in this context indicates that the reciprocity of attitudes merits much more consideration, b) that the definition of self is a prime social phenomenon, which implies for our purposes that the differentiation of attitudes and actions should be related to self-definitions.

The state of our knowledge in the reduction of prejudice and inter-group tensions has been covered in admirable monographs by Rose²⁰ and Williams.²¹ Rather than attempting to summarize their findings, I should like to suggest one possible plan for the observation of inter-group tensions. Three main techniques would be employed: the tension barometer, mass observation, and the area opinionnaire. The tension barometer would depend on the observations of a panel of individuals in specialized employment—police, transit and social workers—as in Detroit. In addition the reporters would give their judgments about the relative state of inter-ethnic relations as well as descriptions of whatever incidents may have occurred. Subsequently these judgments might be used as measures of the reliability of the reporters and their observations could be weighted accordingly. The mass observation team, as nearly as possible representative of the attributes of the areas under study, would be used to report on the state of tension, rather than to seek out specific incidents. All participants would be treated as data as well as fact-finders and changes in their attitudes would be studied as carefully as changes in the areas. Thirdly, to check on the reliability of reports from any area and to give analytic depth, intensive spot opinionnaires would be taken. This kind of a scheme would prevent too heavy reliance

¹⁷ Cited by Quinn McNemar, "Opinion-Attitude Methodology," *Psychological Bulletin*, XLIII (July, 1946), 305.

¹⁸ Presented at meetings of the Western Psychological Society, San Diego, June 20, 1947.

¹⁹ *American Sociological Review*, XIII, No. 1 (February 1948), 36-43.

²⁰ Arnold Rose, *Studies in the Reduction of Prejudice*. Chicago: American Council on Race Relations, 1947. Processed.

²¹ Williams, *Op. cit.*

on a single technique. The description of incidents would validate the purely symbolic indications of tension, and an outbreak of violence would validate a critical point and perhaps even a predictive curve in the three indicators of tension.

I had originally intended in this paper to survey the concepts in general use and to describe with some pride the distance that has been travelled from absolutistic, biologically deterministic notions of social causation.²² Surely the deaths in such a brief span of Park, Reuter, and Thomas is sufficient excuse for retrospection. However, I think they would have it otherwise. Although it is proper and healthy for sciences to look at the road which has been travelled, it is more important to examine the ground under foot. Substantial though our progress may be, we are not going so fast that the starting point will soon be out of sight. What we need is sound judgment about the contours of the road.²³

DISCUSSION

Charles S. Johnson
Fisk University

Professor Bloom's excellent and provocative paper reveals at the very outset the problem of definition, which tends to be both a preoccupation and a weakness in American sociology. For, if there is any need for definition at all, it is to introduce a measure of accuracy which would be inconsistent with his election to use the term "ethnic" in an omnibus sense. Under the definition, a properly conceived ethnic research program, concerned with groups set off from the rest of the population by his various criteria, would include Orientals, Jews, and Negroes, but also Okies and Arkies, because

they are identifiable even though, as he notes, racially invisible. In the same omnibus sense he could as justifiably include the Townsendites, the Aimee McPhersonites, or the migrant Southerners who lack the conspicuous poverty of the Okies and Arkies. In the end they would be "population" studies, which much of our "ethnic" research, no doubt is becoming, without benefit of sociological definition.

Essentially, the phenomenon that is given greatest importance in the elaboration of Professor Bloom's thesis is inter-group relations, which are only confused when contorted into an ethnic framework. The most popular of these ethnic groups for study is the American Negro, and it is ethnic primarily in a biological sense, which sociological inquiry can least affect. On the other hand, the Okies, who are, as he describes them, "racially invisible," bear the social marks, the mental constructs from their isolation, and the temporary disjunction with the surrounding new society that is the essence of the tensions, personality distortions, group conflicts and maladjustments that comprise the chief findings of the best literature of ethnic research.

The disappointment of Professor Bloom that there is no general work covering competently the field of ethnic relations since Young's *American Minority Peoples*, written fifteen years ago, is understandable. Indeed, this may be an encouraging sign of sociology's growing up rather than an indication of its retardation. For, despite the falling off of sharp pointed ethnic studies which deal primarily with non-ethnic differences and distinctions, there has been a flowering of personality and inter-group studies which come closer to the heart of the problem. Insistence upon conceiving present day studies in the cramped mold of fixed ethnic differentiation groups set apart whether by religion, color, or cultural background criteria, is a step backward. It disregards most of what we have learned about acculturation, attitudes, social change, class and bias over the past twenty years.

After this very useful survey by Professor Bloom it appears that what needs to be studied is inter-group problems rather than the ethnic composition of groups. This seems to be the direction of the interesting researches which were instituted under the leadership of the late Dr. Kurt Lewin, not included in Professor Bloom's survey.

The new incursions and interpretations of the

²² But see E. Franklin Frazier, "Sociological Theory and Race Relations," *American Sociological Review*, XII, No. 3 (June 1947), 265-271, and E. B. Reuter, "Developments in the Last Fifty Years: Racial Theory," *American Journal of Sociology*, L (May 1945), 452-461.

²³ It appears at this printing that a long step has been made toward establishing the clearing house for research material called for by several members of the panel. The Committee on Race Relations at the University of Chicago in cooperation with the American Council on Race Relations has just undertaken an inventory of research under way or completed by January 1, 1946.

psychiatrists not given attention in Professor Bloom's paper hold at least as bright promise for the study of inter-group relations as the opinion polls to which substantial space is given.

Progress in the field of research may perhaps be helped some by the refining of ethnic differentiation, but it will be helped vastly more by the refinement of scientific method in dealing with these complex data. This includes test and re-test of results rather than the multiplication of miscellaneous descriptive studies. It includes also the tests of popular stereotypes in the current research methods or findings. Finally, the research will be helped most by the development of more systematic theory and by our ability as sociologists to get behind the obvious and conventionalized modes of social behavior.

DISCUSSION

Ira De A. Reid
Atlanta University

One of the grave concerns in current "ethnic" research is with the theoretical foundations upon which research is built. The most technically competent methods will yield little of value until we re-examine some of the basic theoretical constructs of ethnics and ethnic group relations with which we hypothecate for our methods. The constant sterile repetition of findings of the past has little value in research that seeks the discovery of truth and the attainment of skill mastery in a dynamic society. The central problem in ethnic research continues to be the one of social inter-action with emphasis upon social attitudes (the inter-acting attitudes, of course), inter- and intra-group, and, the institutional structuring of this inter-action. Through an analysis of these aspects of society we might seek to develop calculating rather than descriptive research materials. Through a union of correct conceptual and theoretical analysis and of measurement, the researcher would seek to comprehend the structure of the total situation and the inter-acting forces in it. Such research would have both scientific and operational meaning.

By way of illustration I shall postulate some aspects of theoretical formulations that need re-study and redefinition if they are to be used as basic assumptions for this type of research. They are submitted on the assumption that there may be some unorthodox ways of arriving at truth or of discovering new dependent variables in human relations. As Bloom has indicated,

the uni-directional aspect of attitude studies has indicated the need for measures of reciprocal relationships, especially if one assumes that an attitude *per se* indicates a "readiness for tension." If tension barometers are to be developed as a technically competent method of situational analysis, it seems necessary that we learn how to read, evaluate and predict on the gauge of intra-group controls which form the primary bases for inter-group expressions.

We may go one step beyond this point. Hereafter all of our research in ethnic group relations has been based upon the assumption, or constructed within the framework of, "race," "class" or "caste." As a rigid, inflexible form with solidarity and structural integration, such an assumption seems to permit the use of the concepts of "marginal man," "cultural islands," "Negro," and "white" with a validity not justified by experiential evidence. This seems to call for a re-examination of the empirical materials from which the concepts evolved. A basic problem, it seems, centers around the frame of reference within which we work.

At another point in our research we need to know the elements in the inter-acting groups' behavior that have responsible value for prediction and control. The roles of the religious, economic, and legalistic controls have not yet been studied. Nor have we given basic consideration to in-group-out-group relationships affected by the transition of world societies' composition from one of relatively fixed social status to that of a highly mobile social order.

We might also give consideration to the types of institutions that support the needs of all groups in a population, and those that bolster the activities of only a segment thereof. What institutions actually block social integration? How effectively do they do so? What changes are indicated in contemporary society? How does culture operate to dissipate *common* and promote *like* purposes? Where do "Spanish Americans," or Nisei, or Negroes, or Okies find durable values in a culture of divisively operating institutions?

The institutions and associations of human relations, race relations, intergroup, and inter-cultural relations within our society are new inventions. Are these movements based upon common purposes which have meaning in terms of the deep personality needs of the great masses of people in a political society? Or do they tend to maintain the *idea fixe* of group hierarchies?

Finally, what are the unsocial or asocial behavior patterns that are found extensively in all groups of oppressed peoples or strangers? How is this behavior altered in the process by which the groups move from a relationship of *persecution to toleration* to "so what?"

DISCUSSION

Edgar T. Thompson
Duke University

A sense of problem and a respect for fact are prime requirements for research in any department of science. If the facts exist then they should be gathered, and it is easy to agree with Dr. Bloom that many facts relevant to research in the field of race and race relations have not been gathered. If the facts have been gathered then they should be brought together, and in the field of racial study this has not been systematically done. The Department of Sociology at the University of Chicago should some day carry out its promise of many years standing to serve as a clearing house for old and new material in the field. The periodic publication of fact books is entirely feasible. If the facts have been brought together then they should be released and made available, and Dr. Bloom indicates that certain facts now are being withheld.

All this is obvious. It ought to be equally obvious that the range of fact-gathering should be as extensive as possible, for nothing less than the perspective of a world-view is sufficient to enable us to detach ourselves from the merely local and provincial—and the idea of race has long ceased to be a local idea. Such studies as we have from Hawaii, Brazil and South Africa have taught us that the more we study variant racial situations in different parts of the world the more clearly do we understand our problems within the United States. It begins to appear that around the world there are race relations' "regions," regions in which relations between peoples regarded as racially different are more or less problematic and more or less involved in the existing political and social problems. We are aware of the fact that in the United States there are areal differences in the patterns of race relations but there is also sufficient similarity between these patterns, as McWilliams has shown, to point up the United States as a race relations' region from the world-community standpoint. Various indexes might be devised to delineate and map these regions

much as ethnologists seek to map culture areas. The presence of a population regarded as "mixed-bloods" might constitute one such index. At any rate, however these regions are defined, such basic facts as exist about them should be brought together and systematized. The result, I feel sure, would greatly facilitate the comparative study of race relations.

The time for the investigator to worry about the facts, however, is when he senses or knows what his problem is, and I may venture the suggestion that our chief concern right now is not the adequacy of facts but rather the adequacy of theory. Of course, Dr. Bloom is aware of all this for he speaks of the "danger of purposeless fact-gathering and theoretical sterility" but he does not indicate the extent to which our conventional theory and modes of research are sterile when applied to the existing mobile and highly unstable situation.

I am of the opinion that much if not most contemporary research in the field of race relations is proceeding within the framework of a body of social theory which has been strongly influenced by social anthropology and ethnology and which is chiefly concerned with established structures in relatively static societies and with the behavior which fits into those structures. The effect of tradition working in and through these structures has been to make social behavior more or less predictable, a fact which has appealed to the scientific aspirations of sociologists. Now it is surely important to know how any given society is or has been structured. The Negro and the white man, and especially the Southern white man, for example, have both to be understood within the context of the social order in which they have lived and had their being. The very first thing to be understood is the institutional order within which two peoples subsequently defined as racially different came together and reacted upon each other. All changes in the personalities and conditions of whites and Negroes cannot properly be understood except when measured against the background of institutional authority and law. We have well understood this and most of the facts which we have been concerned to gather have been those which have to do with organized arrangements and customary routines. Of course, great gaps continue to exist in the factual basis of our knowledge concerning these institutional structures. In the census statistics on the South, for instance, "the plantation does not exist; . . . it lives statistically under the disguise of its

direct competitor and adversary, the small family farm."¹

Now the great body of existing social theory developed in an effort to explain traditional structures and to describe the change from historical structure to historical structure. Relatively little attention has been given to the processes by which these changes have taken and are taking place. The very wide sense in which Dr. Bloom uses the term *ethnic* is subject to criticism but it does serve to suggest that peoples differentiated from each other on almost any basis are potentially racial groups. However, we know little or nothing about the process by which they become such and we know next to nothing about the types of race-making situations in which the process operates. Negroes and whites in early Virginia were not originally defined as racially different but as the plantation developed they came to be so regarded. Very much the same result seems to have occurred in the case of Englishmen and Italians in Queensland, Australia. And if relations between capitalists and laborers in Western Europe and the United States have not yet been defined as race relations there is certainly a clear tendency in contemporary left-wing literature to depict the capitalist as a man of virtually another race, another blood.² What is the process by which people come to leave the shells of old structures and to incorporate themselves in new ones? When and under what circumstances does social differentiation take a racial trend? What factors must be present in the situation in order that this will occur?

It is with respect to the new world into which we are moving that one most feels the inadequacy of our conventional ideas and modes of research and the need for a new set of guiding principles. Modern society, as contrasted with primitive and folk societies, is relatively without tradition. Men are everywhere groping for new satisfactions that cannot be gained from existing arrangements. Much of their behavior represents conduct in formation and is therefore unpatterned and unstructured. One of the consequences of the collapse of old institutional structures is the rising flood of news we are getting, news that depicts unexpected behavior. Such knowledge as we have believed ourselves to

have is failing, both the common sense and scientific types of knowledge. In his review of Franz Boas' *Race and Democratic Society* Leslie A. White commented

If any one problem could be singled out from the many with which Boas concerned himself and called the major interests of his scientific life it would, I think, be the race question. He gave countless lectures and wrote scores of articles on the subject. . . . Yet it cannot be said that Boas ever gave an adequate scientific explanation of racial antagonisms, and I know of no evidence that will show that all of his efforts diminished race prejudice by one iota. . . . Boas' attack upon race prejudice is like an attempt to rid a psychopath of the delusion that he is Napoleon by demonstrating that his belief is scientifically—chronologically, anthropometrically, linguistically—unsound. But the psychopath's delusion arises from other sources and causes; it merely expresses itself in this particular form.³

Boas founded his doctrines in theory which not only failed to provide a basis for control but failed also to provide a guide for significant research. Current social theory likewise does not appear to be leading us toward an ordered understanding of race relations as these relations are daily being reformed in our dynamic, highly mobile modern society. We are living in a world of swift and universal change, in an atmosphere charged with doubt and apprehension, so permeating that it reaches everywhere. I know we have heard such remarks before but current racial theory has not yet come to terms with the fact. Myrdal's attack on the concept of mores went, in my opinion, too far but I think it is symptomatic of dissatisfaction with the results of the application of conventional theories to research in the field of race relations. Judging by the extent to which we seem reduced to repeating and paraphrasing each other and quibbling with each other over such questions as whether or not race relations are caste relations it would appear that the perspective of more adequate theory to guide the empirical consideration of race relations in the dynamics of modern life is sorely needed.

DISCUSSION

Robin M. Williams, Jr.
Cornell University

Professor Bloom has stressed the need for a comprehensive series of descriptive studies

¹ Karl Brandt, "Fallacious Census Terminology and Its Consequences in Agriculture," *Social Research*, V (February, 1938), 22.

² Gaetano Mosca, *The Ruling Class* (New York: McGraw-Hill Book Co., 1939), p. 307.

³ *American Journal of Sociology*, LII (January, 1947), 371.

which would painstakingly detail for us the actual patterns of intergroup behavior. Probably most social scientists who have worked in the field now under discussion will agree that the available descriptive materials about some very important groups are pathetically scanty and distorted. Furthermore, even a man who got "the cart before the horse" (with very little effort, I might add) can and does agree that these inadequacies will often render it wasteful or unproductive to attempt elaborate field experiments. With regard to this point, however, there is more to be said: (1) The relative amount of attention to be given to "description" or to something else is a problem of how we now use our limited time, talents, and funds. In spite of deficiencies in description, there definitely are many situations in which important control-variables can be specified accurately enough for useful field experiments or controlled comparisons. (2) Even where an adequate descriptive framework is not yet available, there is no compelling reason why a given study has to content itself with a "community description," and stop there. There seems more reason to suppose that such monographic projects would gain immensely by going on to devise comparative (experimental or quasi-experimental) analyses within the situation already portrayed.

Undoubtedly we need to know a bald eagle from a sparrow before we concentrate on, say the migratory habits or physiology of these animals; we must first be able to distinguish various varieties of ethnic relations before getting very elaborate or pretentious about more subtle problems. I think however, that: (1) there is already in existence a body of knowledge of very respectable proportions concerning ethnic problems; (2) description "on a low level of conceptualization" has definite advantages which should not be minimized, but these must be set over against the impressive sterility of many ambitious but relatively "non-directional" data-gathering sorties. Thirdly, it seems to me very doubtful whether as a practical matter sociologists can avoid the task even if they should try, of dealing with problems of social action which require for their solution at least a logical equivalent to controlled experimentation. We need to know what groupings exist, where they are, what behavior is involved. But we urgently need also to know some answers to questions which begin with "what happens if you . . . do so-and-so?" And to these questions we must bring not only meticulous, repeated,

and comprehensive description, but also some generalizations as to the probable consequences of given actions in certain sorts of situations.

The nation-wide program of research proposed by our speaker to build up a synoptic picture of American ethnic relations, is clearly a long-term matter; in so far as such a program is implemented productively it will require the participation of workers from all fields of social science. Searching, multi-dimensional analysis will be necessary to make the most of the descriptive survey. Sociologists and anthropologists have long been proponents of the view that a social system must be seen as a whole if there is to be scientific understanding of the repercussions of seemingly local and partial processes. Yet much ethnic research has reified an entity called "prejudice" and treated it as if it empirically operated in isolation from its context in a total structure of motivated human action. We must seek out the structural (recurrent) sources of aggressive needs and relate patterned hostility to the moving equilibrium which defines a society in operation. Where this has been done, concretely and in detail, it will be possible to speak with some measure of scientific assurance in appraising the probable consequences of various public policies or action programs.

In broadening the conception of "ethnic" beyond groupings based on racial visibility, certain cautions are in order. It is well to recognize similarities in such social categories as Negroes, women, Oakies, physically handicapped, Jews, Catholics, aliens, and so forth. On the other hand, we shall have to continue to make explicit the very important differences found in relations oriented to these diverse criteria of grouping. The most important axis of differentiation probably is that of achieved vs. ascribed status, with all the further complexities inherent in these very broad concepts. In short, although the implication is not in Professor Bloom's paper, it seems worth while to indicate that there is little utility in defining ethnic relations so broadly as to make them synonymous with in-group versus out-group relations.

On the other hand, I would like to suggest that some of the most important contributions to our knowledge of ethnic problems are likely to come from fundamental comparative analyses of the *general* mechanisms of social solidarity. One of the most basic questions in the field of ethnic problems, for example, is: to what extent and in what ways, if at all, is out-group aggression *necessary* for social solidarity? What con-

ditions promote a "positive" solidarity rather than the unity of a common dislike? Given the existence of intragroup hostility, what factors determine the form and direction of its release? Greater scientific understanding of such questions seems to be rapidly becoming a prerequisite for human survival.

In addition to this "fundamental" level of research, we greatly need analysis of the strategy and results of purposive ameliorative action. I, for one, would regard this area as being currently at least as promising as general descriptive study.

One final observation has to do with the basic nature of the phenomena we are discussing. The members of this panel seem to agree that ethnic prejudice is not to be understood except as a living reality of functioning groups. In the modern world of rapid change and dissolution of the deeper mechanisms of institutionalized regulation, an impressive additional fact is the prevalence of much apparently amorphous, free-floating aggression, which is readily mobilized toward the most diverse targets. This is not "traditional" prejudice, but is perhaps even more dangerous. We greatly need further incisive research to identify the sources of this polymorph hostility and to explore possible lines of control.

DISCUSSION

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Implicit in Dr. Bloom's statement is the proposal of a coordinating council to assist in the formulation of the studies mentioned. Too long we have operated *exclusively* on the principle that every scholar must be allowed complete freedom to set up and conduct any research program that happens to strike his fancy and for which he can somehow obtain funds. The result is the bewildering collection of unrelated and undigested facts which now substitute for knowledge in much of the field.

What is needed to bring order out of the present chaos is not additional facts, acquired in the same haphazard fashion as in the past.

We do need desperately, it seems to me, an additional body of basic theory by which to guide the collection of facts and itself to be tested by such empirical research. I have in mind a set of propositions as simple in expression and yet as full of insight and as universal in their application as Park's formulation of the race relations cycle or Reuter's theory of the natural history of race relations. Such hypotheses to be of any consequence, it goes without saying, must be derived from an intensive and well-rounded acquaintance with at least *one* area of inter-ethnic relations and a less intensive comparative study of a number of typical situations. The departure of such trail-blazers as Park and Reuter and Thomas, far from being merely "an excuse for retrospection," should stimulate a fresh outburst of theory among their disciples and their critics. The time may well be ripe for the setting up of a simple device of intercommunication and interstimulation among students of racial and cultural relations comparable to that established by the Social Science Research Council some years ago for the formulation of a common body of theory on acculturation.

Dr. Bloom's concern regarding the refinements in methodology—his emphasis upon "the detailed and exact analysis of the differentiation of ethnic groups"—for example, succeeds, it seems to me, in placing the cart precisely before the horse. The prior and more difficult problem relates to theory. Once we have made clear in our own minds what it is important to know about ethnic relations, the problems of methods and of techniques will not be insurmountable, although they will still be important, it is true.

The proposed series of locality studies, if conducted within a common conceptual framework, might afford the means of testing a variety of hypotheses within the field of racial and cultural relations. If carefully conceived, within a framework comparable to that used by Redfield in his study of Yucatan communities, the number of such points of intensive investigation would not need to be large nor the cost excessive.

SOCIAL PSYCHIATRY*

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A REVIEW of the contacts between psychiatry and sociology in the United States¹ points to the fact that a field or research area called "social psychiatry" has emerged even though somewhat fitfully. This has resulted from the attempt to study certain problems considered as psychiatric from the point of view and with the techniques of the sociologist. A semantic difficulty immediately arises, however, when it is recognized that other "social" disciplines,

namely psychoanalysis, psychology, and anthropology, also have studied psychiatric problems from their respective viewpoints and with their various techniques. It is a difficulty because it questions the proposition that a field of social psychiatry should be regarded as exclusively the product of the sociologists.

The more formal professional relations between psychiatry and sociology during the past twenty years as reflected in committee reports, joint meetings, and annual programs, only serve to publicize a relationship which, at least in the minds of certain members of the respective societies, had been present for some time.² True, the initial psychiatric influence in sociology had come via psychoanalysis and true also that among the psychiatrists there were research-minded men of the high caliber of W. A. White, William Healy, Trigant Burrow, Charles Campbell, and Adolph Meyer, who in their researches were increasingly coming up against problems which were not only sociological in character, but which demanded the specialized training of a sociologist.

CONVERGING PERSPECTIVES

The question as to whether a field of social psychiatry has been developed is certainly a moot one. It may be possible to come closer to this issue by examining the kind of psychiatric interest manifested by the various social disciplines tangential to psychiatry. In so doing, we will also be able to evaluate any conception of a "social psychiatry"

*Paper read before the annual meeting of the American Sociological Society, New York City, December 28-30, 1947.

¹The first indication, perhaps, of a psychiatric influence on sociology came in 1917 when Groves' article, "Sociology and Psychoanalytic Sociology," appeared in the *American Journal of Sociology* (Vol. 23, 1917, pp. 107-116). At the annual meeting in 1920 Groves and Gehlke jointly presided at a roundtable on the "Sociological Significance of Psychoanalytic Psychology." This interest was not continued in 1921, but in 1922 a new section "Psychic Factors in Social Causation," appeared on the program, at which Ogburn presented a paper, "Psychoanalysis and the Subjective in Relation to Sociology." From then nothing of significance happened until at a meeting of the Executive Committee of the Society on December 29, 1927, President W. I. Thomas read a statement from Dr. Harry Stack Sullivan of the American Psychiatric Society, which suggested that a committee from the American Sociological Society be appointed to confer with a committee from the American Psychiatric Society on plans for promoting their joint common interests. Such a committee was appointed.

At the next annual meeting, the committee on the Relations of Sociology and Psychiatry (W. I. Thomas, Kimball Young, and R. E. Park) reported that a joint colloquium on personality investigation was held in New York City. In addition, the Committee on Sections reported that "Psychiatry and Sociology" met for the first time under the chairmanship of the late Professor Robert E. Park. This section continued to meet, except in 1934, as part of the annual program until 1941, when it met for the last time. At the annual meetings in 1943 and 1946, this section did not meet. Only once, in 1932, under the chairmanship of Thomas Eliot was the section labelled "social Psychiatry" instead of "sociology and Psychiatry."

²William White presiding at the First Colloquium on personality investigation stated in his opening remarks, "... but fortified by the profound conviction that has been forced upon us for many years that psychiatry, with the material it has to deal with, is dealing with conditions that are essentially different from the materials which general medicine deals." See "Proceedings of First Colloquium on Personality Investigation," *American Journal of Psychiatry*, 8 (1928-29), 1019-1177.

emerging from these disciplines or held by them.

It can, no doubt, be said in fairness to all of the disciplines concerned, that the workers in sociology have been the most avid and aggressive in attempting to mark out the boundaries of such a field. Brown, Dunham, Folsom, Groves and Krout have written articles dealing with the supposed field of social psychiatry.³

From these authors, three emphases might be said to emerge. These are (1) the attempt to relate the field of social psychiatry to general psychiatry and the larger area of psychopathology (Krout); (2) to point out some of the problems which make up social psychiatry and which deal with the possible relationship of some environmental variable to some psychiatric syndrome; (Brown, Dunham, Folsom); and (3) to show the converging research lines of development making for a social psychiatry. (Groves)

In terms of research⁴ it seems that the field of social psychiatry has encompassed studies showing correlations of certain personality disorders or maladjustments with

some variable derived from the social environment. To sum up, one might say that the sociologist has attempted to carve out and delimit a so-called field of social psychiatry⁵ in a direct proportion to the research effort expended on satisfactorily explaining personality disorders and group maladjustments.

In contrast to the sociologist, the cultural anthropologist has shown no need to develop or even to speak about a "social psychiatry." On the one hand, he has been busily engaged in incorporating certain psychoanalytic principles into his thinking in order to sharpen his analysis of behavior and personality in alien cultures. On the other hand, he has been concerned with the necessity to synchronize psychoanalytic armchair anthropology with field research anthropology concerning the genesis and nature of human culture. Kluckhohn has given a competent and detailed account of the reciprocal influence of anthropology and psychiatry in the United States.⁶

Perhaps, more than either the anthropologist or the sociologist, the psychologist has felt a constant and continuing relationship with the kind of problems faced by the psychiatrist. The numerous textbooks of abnormal psychology written by both psychiatrists and psychologists demonstrate this mutual inter-penetrating interest.⁷ Then, too, in their research—omitting from consideration animal psychology—the psychologists have attempted to study objectively (1) the

³ See L. Guy Brown, "The Field and Problems of Social Psychiatry." In *The Fields and Methods of Sociology*, L. L. Bernard (Ed.) Ray Long and Richard R. Smith Publishers, New York: Part I, 129-145. H. Warren Dunham, "The Development of Social Psychiatry," *Mental Health Bulletin*, Illinois Society for Mental Hygiene, XVIII, (March-April 1940) 4-7.

Joseph K. Folsom, "The Sources and Methods of Social Psychiatry." In *The Fields and Methods of Sociology*, L. L. Bernard (Ed.) Ray Long and Richard R. Smith Publishers, New York: Part II, 387-401. E. R. Groves, "The Development of Social Psychiatry," *Psychoanalytic Review*, XXIII (Jan. 1935) 1-9. Maurice H. Krout, "The Province of Social Psychiatry," *Journal of Abnormal and Social Psychology*, XXVIII (1933-34), 155-159.

⁴ The account of the developments in social psychology during the thirties by Cottrell and Gallagher has included many of the newer viewpoints and examples of research which have come to be regarded as a part of social psychiatry. Cottrell and Gallagher include them in their article because of the broad encompassing conception which they think is expressive of contemporary social psychology. See their *Developments in Social Psychology* (1930-1940), Sociometry Monograph No. 1, 1941, pp. 1-58.

⁵ It is of passing historical interest to note that the term, "social psychiatry," is used by the *American Journal of Sociology*. See "Selected References on Social Psychiatry," XLII (May 1937), 892-894.

⁶ "The Influence of Psychiatry on Anthropology in America During the Past One Hundred Years," in *One Hundred Years of American Psychiatry, 1844-1944*. New York: Columbia University Press, 589-617.

⁷ For such examples, see Bernard Hart, *The Psychology of Insanity* (Fourth Edition), New York: The Macmillan Company, 1931. A. Myerson, *The Psychology of Mental Disorders*, New York, 1927. R. M. Dorcus and G. W. Shaffer, *Textbook of Abnormal Psychology*, Baltimore, Williams & Wilkins, 1934. William McDougall, *Outlines of Abnormal Psychology*, New York: Charles Scribner's Sons, 1926.

specific mental mechanisms of man, (2) the development of the child in society and (3) the nature of personality—all of which are of interest to the psychiatrists. These three areas of interest frequently involved the abnormal and bizarre in mental reactions as well as the maladjustments of persons living in society. The psychologists were quick to develop their own theories for certain mental diseases and to amass a body of evidence which served to support their theories.⁸ Some of their work here is reflected in the various tests developed which were diagnostic of personality traits, intellectual traits, and sometimes psychiatric syndromes. Unlike the sociologists, who only recently have shown specific interests in the abnormal mental life, the psychologists have had no need to carve out any special field to encompass their research interests as the field of abnormal psychology always proved adequate.

Psychoanalysis in its orthodox form⁹ has succeeded in building a closed system of psychological psychiatry. It has a theory of the growth and development of the human personality which is deterministic, dynamic, substantial, exclusive and pragmatic. It has a system of therapy which rests upon its body of theory. Like any body of scientific

knowledge, it began with certain problems and emerged as a body of theory and generalizations for explaining those problems. Through its literature, techniques, therapeutic results, and propaganda, it has established itself as a branch of medicine and particularly of psychiatry.¹⁰

Psychoanalysis through its theory, its research techniques, its psychologism and its intellectual pretensions in other scientific areas has had a much closer intellectual affinity with the social sciences than with the biological and natural sciences. Specifically, it aligns itself with social psychology and sociology in its emphasis upon the family constellation and its account of the emotional ties between fathers and daughters and mothers and sons. Again, in placing emphasis upon the errors of every-day life and upon the meanings of dreams, psychoanalysis was calling attention not only to the unconscious in which these forgotten experiences were hidden, but also to the emergence, form and content of human experience as it took place in a social context. What could be more social psychological than this?

Again, specifically, psychoanalysis branched out in its system of sociology¹¹ and challenged certain anthropological conceptions of the origin, nature and functioning of human culture. The complete biologism that all human institutions were basically to be understood as being genetically an outgrowth of man's instinctual life was unsatisfactory in its failure to square with the empirical

⁸ See M. Sherif and H. Cantril, *The Psychology of Ego Involvements*, New York: John Wiley and Sons, Inc., 1947. See especially Chaps. 12 and 13. G. W. Kisker and G. W. Knox, "The Psychosocial Basis of Mental Disorder," *Journal of Nervous and Mental Disease*, 93 (1941), 163-168. G. W. Kisker and G. W. Knox, "The Psychopathology of the Ego System," *Journal of Nervous and Mental Disease*, 96 (1943), 66-71; "The Psychologist's Contribution to the Study of the Mental Patient," *Journal of Personality*, 15 (Dec. 1946), 93-141.

⁹ In speaking of the orthodox form of psychoanalysis our intention, of course, is to refer only to the views of Freud and his intellectual advocates. We are well aware that certain of Freud's rebellious sons, Adler, Jung, Rank, and Stekel, have in their writings exerted definite influences on social science. Jung's "psychological types" and Adler's concepts of "organ inferiority" and "style of life" have frequently been useful in sociological analysis. However, we feel that to discuss all of these influences, in detail, significant as some of them are and may prove to be, would carry us beyond the scope of this paper.

¹⁰ See for example, A. A. Brill, *Freud's Contribution to Psychiatry*, New York: W. W. Norton and Co., 1944. See also Theodore Van Schlevlen, "Psychiatry and Psychoanalysis," *International Journal of Psychoanalysis*, X (1929): Sandor Rado, "Psychoanalysis and Psychiatry," XVII (1936), 202-205. Also Leland Hinsie, "The Relationship of Psychoanalysis to Psychiatry," *American Journal of Psychiatry*, 91 (1935), 1105-1115. These references are only suggestive. The literature is replete with articles dealing with this relationship.

¹¹ Freud's sociological system is found in the following works: S. Freud, *Totem and Taboo*, New York: Moffat, Yard and Co., 1918; *Group Psychology and the Analysis of the Ego*, London, 1922; *The Future of an Illusion*, London, 1928; and *Civilization and Its Discontents*, London: Hogarth Press, 1930.

formulations of social scientists. The resulting critical appraisal by the social scientist of Freudian sociology produced a situation of mutual inter-stimulation between the psychoanalysts and cultural anthropologists. Thus, at the points where its theory cuts across or into the theory of other scientific disciplines about psycho-social reality and in the attempts to square its theory with other competing or existing theories, psychoanalysis enters into and becomes a part of that body of scientific knowledge which seeks to explain the bizarre and peculiar psychic states of man and the difficulties of behavior in man which can be observed as a distorted aspect of the rubric of social life. Psychoanalysis as a dynamic psychology of mental life must mesh eventually with a dynamic sociology of inter-personal and cultural relationships.

We have been concerned with the respective roles of sociology, cultural anthropology, psychology, and psychoanalysis in relation to the problems presented by the neuroses, psychoses, and other psychopathies among humans in society. Thus, these disciplines have tended to cope with certain problems relating to the aberrations of man's mental life which traditionally have been regarded as the province of psychiatry. Now, psychiatry, narrowly defined, is that branch of medicine which deals with the study, diagnosis and treatment of mental disturbances and diseases. However, this narrow conception of psychiatry proved in many instances singularly unsatisfying to the intellectual leaders in the field as they began to contemplate the character of their problems and materials and to glimpse the research and thinking about the neuroses and psychoses which were being carried on by other scientific fields. Two recent emphases can be cited (although there are others) of the attempt of psychiatry to get out of its traditionally rather tight clothing. The first is found in the writings of Harry Stack Sullivan. Sullivan sees modern psychiatry as a union of two strains of thought—one leading back to the Hippocratic school of medicine and concerned with the art of observing and treating mental disorders—and the other strain of

thought concerned with the genesis of man as a social being. In psychiatry the three men who helped to bring these two intellectual strains together were Freud, Meyer and White. But Sullivan, adds,

This synthesis is not yet complete. The next, I trust, great step in its emergence came with the realization that the field of psychiatry is neither the mentally sick individual, nor the successful and unsuccessful processes that may be observed in groups and that can be studied in detached objectivity. Psychiatry, instead, is the study of processes that involve or go on between people. The field of psychiatry is the field of interpersonal relations, under any and all circumstances in which these relations exist. It was seen that a *personality* can never be isolated from the complex of interpersonal relations in which the person lives and has his being.¹²

Thus, Sullivan would extend the conception of psychiatry so that it is coterminous with much of what sociologists regard as the field of social psychology today. In fact, many of Sullivan's concepts such as empathy, social acts, the self, the personal world, and social interaction are reminiscent of the older social thought of Cooley, Dewey and Mead.

A second attempt to broaden the traditional conception of psychiatry is shown rather clearly by Masserman's recent textbook in the field. On the first page of this work is to be found the statement, "Psychiatry can be broadly defined as the science of human behavior."¹³ Psychiatry is apparently to usurp the role—which sociology was at one time pressed to occupy—as the great synthesizing science of all the sciences dealing with human behavior.

Now, it is not our intention to be critical of these broadening conceptions but rather to point to the fact that they were inevitable

¹² "Conceptions of Modern Psychiatry," *Psychiatry*, III (Feb. 1940), 4-5.

¹³ Jules H. Masserman, *Principles of Dynamic Psychiatry*, Philadelphia: W. B. Saunders Co., 1946, p. 1. Along a similar line Zilboorg argues that psychiatry must create its own sociology and that clinical psychiatry has partially succeeded in doing this. See Gregory Zilboorg, "Psychiatry as a Social Science," *American Journal of Psychiatry*, 99 (Jan. 1943), 585-588.

—inevitable if it is recognized that the kind of data with which medicine traditionally had to deal was neither sufficient nor useful in answering all of the questions which psychiatrists were asking. As the social sciences have reached out to study problems of a psychiatric nature, so has psychiatry extended itself to embrace the methodological positions, data and techniques of those social disciplines.

We have now come to a point in the development of our thesis where we can raise the question: "What is social psychiatry anyway?" The answer is that it is pretty much a creation of the sociologists to designate the interests of certain of their numbers who are doing research in the field of personality disorder or, following Folsom, are trying to find meaningful correlations between various types of personality disorders and certain variables in the realm "of social interaction, social environment, or culture."¹⁴

But the anthropologist and the psychologist have also been concerned with these interests and the development of similar correlations. Workers in these fields are no less working in "social psychiatry" even though they have not applied the label. The term, social psychiatry, thus appears to be at best a misnomer or at the worst an unfortunate term when linked in any exclusive sense with sociology. It is certainly not "psychiatry" even though the problems as represented by the neuroses and psychoses may have traditionally belonged to the field of psychiatry. It is certainly not "social" merely because sociologists have done some of the work in any greater sense than would be other such research work which might be carried on by anthropologists, psychologists, or psychoanalysts.

What has happened is that the kinds of problems present in the field of psychiatry are ones which cannot be approached and solved exclusively within the framework of the biological sciences. For in the psychiatric sphere particularly medicine needs the cooperation, the insights and the viewpoints

of the social sciences.¹⁵ This position is all too clearly reflected in the attempts already described of Sullivan and Masserman to extend the traditional boundaries of psychiatry.

What seems to be developing in terms of a broader psychiatry and the research concerns of these other disciplines is the general recognition among workers in these various fields that there exists a particular constellation of problems centering around the deviant character of various mental processes in man, the deviant character of man's behavior, and the role and function of such deviations as they evolve in society. Such deviations which we have broadly described run all the way from mild peculiarities of behavior to the extreme forms of psychoses.¹⁶

In this range of behavior forms is a set of problems which require the attention both independently and cooperatively of workers in the social sciences as well as in the biological and medical sciences. In attempting to carve out such a field, we have before us an analogous situation represented by criminology. Here is a scientific field concerned

¹⁵ See Herman M. Adler, "The Relation between Psychiatry and Social Sciences," *American Journal of Psychiatry*, VI (April, 1927), 661-670. It is of interest to note that Adler considers the psychiatric social worker as the practitioner for psychiatric problems within the social sciences. The same position is somewhat implicit in Samuel W. Hartwell's, "Social Psychiatry—Our Task or a New Profession," *The American Journal of Psychiatry*, 19 (March, 1940), 1089-1104. Hartwell makes a plea for the adequate training of psychiatric social workers in order to make them real assistants to the psychiatrists. Incidentally, this is the only time to our knowledge that a psychiatrist has used the concept, "social psychiatry," and this in a context completely different from that in which the sociologists have used it.

¹⁶ While we are not committed here to regarding all forms of abnormal behavior as constituting one continuum, it is of interest here that a psychiatrist writes, "The conviction is becoming widespread among psychiatrists, psychologists, and sociologists, that the same kind of personal, social and environmental factors are operating in preparing the ground for and in precipitating the functional psychoses as in the case of the neuroses." See N. Cameron, "The Functional Psychoses," in *Personality and Behavior Disorders*, (J. McV. Hunt) Vol. II, New York, Ronald Press Co., 1944, 861-921.

¹⁴ See J. Folsom, *op. cit.*, p. 387.

with explaining various kinds of behavior which do not have sanction by the dominant group in human society. In addition, the field is concerned with the handling, control and rehabilitation of such persons. Now the valid and reliable knowledge in this field has been built up by the joint efforts of sociologists, psychiatrists, anthropologists, psychologists, psychoanalysts and other workers.¹⁷

It is precisely something analogous to criminology which is pressing for emergence via the problems of etiology, control, and therapy of those persons whose mental peculiarities make them strange to other persons in society. The label for such a field is much less certain than the fact of its existence as attested by the research contributions to this problem area by the various social, psychological and biological sciences. No doubt, its eventual label will be dependent upon the reliability and validity of the knowledge which eventually emerges from the researches of the various sciences involved.

We would now like to consider some of the specific kinds of knowledge which have been developed by the sociologists and cultural anthropologists during the last fifteen years¹⁸ and which in the hands of the sociologists have been labelled "social psychiatry." It is not our intention to cite every isolated piece of research¹⁹ which has been reported but (1) to point out the major areas de-

veloped, (2) to give some evaluation of their significance, and (3) to show the emergence of certain crucial problems. During this period there seem to have been four general areas of research which have been exploited and which have yielded results of varying significance. These four areas include studies which can be designated as: (1) ecological and statistical, (2) personality and culture, (3) caste and class, and (4) interpersonal relations.

ECOLOGICAL AND STATISTICAL STUDIES

Since 1939 when Faris and I reported on the results of our ecological studies of mental disorder in Chicago and Providence,²⁰ a number of other similar studies have appeared which have served as a check on our original findings. Reference is made to the studies of Green,²¹ Mowrer,²² Queen,²³ Schroeder,²⁴ and Hadley.²⁵ The major findings of our ecological studies are fairly well known and there is no intention of reviewing them here.²⁶ Rather, our concern will be to note the extent to which the other studies

logical Implications of the Neuroses," *Journal of Social Psychology*, XV (1942); Freda Fromm-Reichmann, "Remarks on the Philosophy of Mental Disorders," *Psychiatry*, IX (November, 1946), 293-308.

²⁰ See Robert E. L. Faris, and H. Warren Dunham, *Mental Disorders in Urban Areas*, Chicago: University of Chicago Press, 1939.

²¹ H. W. Green, *Persons Admitted to the Cleveland State Hospital 1928-37*, Cleveland Health Council, 1939.

²² E. Mowrer, "A Study of Personal Disorganization," *American Sociological Review*, IV (August, 1939), 475-487. See also his *Disorganization—Personal and Social*, New York: J. B. Lippincott Co., 1942, Chapters 15 and 16.

²³ Stuart A. Queen, "The Ecological Studies of Mental Disorder," *American Soc. Rev.*, V (April, 1940), 201-209.

²⁴ C. W. Schroeder, "Mental Disorders in Cities," *American Journal of Sociology*, 47 (July, 1942), 40-47.

²⁵ E. E. Hadley and Others, "Military Psychiatry—An Ecological Note," *Psychiatry*, VII (November, 1944), 379-407.

²⁶ For an excellent review, see R. E. L. Faris, "Ecological Factors in Human Behavior," In *Personality and Behavior Disorders* (J. McV. Hunt, Ed.) Chapter 24, New York: The Ronald Press Co., 1944, 736-757.

¹⁷ In Great Britain a post-war institution has evolved which brings together all the social sciences, including psychiatry, for a joint attack on social problems. See Jacques Elliott, "Some Principles of Organization of a Social Therapeutic Institution," *Journal of Social Issues*, III (Spring, 1947), 4-10.

¹⁸ Thus, we are dating this from 1932, when the status of the various fields of sociology was appraised at the annual meetings. See L. Guy Brown, "The Fields of Social Psychiatry," and Joseph K. Folsom, "The Sources and Methods of Social Psychiatry," *op. cit.*

¹⁹ We are concerned here largely with the recent researches of the sociologists and cultural anthropologists. However, many psychiatrists and psychoanalysts through their writings and researches are contributing to the building of a "social psychiatry." See for example, James Plant, *Personality and the Cultural Pattern*, New York, 1937. Paul Schilder, "The Social Neurosis," *Psychoanalytic Review*, XXV (January 1938), 1-19; Paul Schilder, "The Socio-

check and agree with the findings for Chicago. There seems to be one point upon which all the studies are in agreement, namely, that all types of mental disorder in their distribution in the city show a wide range of rates with the high rates concentrated at the center of the city and declining in all directions toward the periphery. Thus, Schroeder concludes in his summary of the evidence that "insanity areas" have been shown to exist. Ecological distributions of mental disorders in nine cities support this finding.

However, while some agreement exists, it is by no means universal with respect to the distribution of the various kinds of psychoses. Our major finding concerning the difference in the distribution of schizophrenia and manic-depressive psychosis has not been conclusively substantiated. There is still less agreement with respect to the toxic and organic psychoses. The correlation which we originally reported between many of the distributions and various indexes of socioeconomic level and social solidarity are likely to show marked shifts if new samples are secured. Queen²⁷ has pointed to the need for more refined indexes for measuring specific community conditions and for investigators to agree on the same index in order to facilitate comparisons.

Various explanations for the patterns of the rates as found have appeared from time to time. These include the "selective character of cases going to state hospitals," the "drifting hypothesis,"²⁸ the "lack of significant statistical difference between the rates,"²⁹ and the "statistical illusory quality of the rates due to mobility."³⁰ Answers have been given for all of these critical hypotheses and, if they have not been conclusively an-

nihilated, much doubt has been cast on their validity for disposing of the significance of the various rate patterns, namely, the pointing to the role of the social environment as a broad, general etiological agent for these various disorders.

In a recent article³¹ I have already set down my chief criticism and evaluations of these ecological studies and so, now, I wish merely to quote the final paragraph.

... Queen and others point to the need for more complete life histories of persons who develop these psychoses in contrast to those who do not in the same community setting. Perhaps, but one can never know this, the ecological studies may have stimulated demands for the above type of study. Be that as it may, the concluding note would appear to be that these studies have provided important and useful information about our community life; they have revealed little that is significant about the etiological factors which lie behind the various types of mental disorder.

Let us now turn to a consideration of studies of the distribution of mental disorder through time. These studies are of less value than those of spatial distribution. This is due to the lack and inadequacy of the statistics over long periods of time in different places. The pressing question as to whether mental disorder is increasing cannot be satisfactorily answered. Winston,³² Dorn,³³ and Elkind³⁴ question an increase while Malz-

²⁷ See H. Warren Dunham, "The Current Status of Ecological Research in Mental Disorder," *Social Forces*, 25 (March, 1947), 321-326.

²⁸ E. Winston, "The Assumed Increase of Mental Disease," *American Journal of Sociology*, 40 (January, 1935), 427-429.

²⁹ H. F. Dorn, "The Incidence and Future Expectancy of Mental Disease," *Public Health Reports*, 53 (November 11, 1938), 1991-2004.

³⁰ Henry B. Elkind, and M. Taylor, "The Alleged Increase in the Incidence of the Major Psychoses," *American Journal of Psychiatry*, 92 (January, 1936). See also J. S. Jacobs, "A Note on the Alleged Increase of Insanity," *Journal of Abnormal and Social Psychology*, 23 (1938), 390-397. Jacobs argues that an increase in the number of beds accounts for an increase in first admissions. I find it generally difficult to accept this argument because it seems to imply that an increase in the number of beds causes us to detect cases in order to fill the beds rather than

³¹ *Op. cit.*

³² See review of *Mental Disorders in Urban Areas* by A. Myerson, *American Journal of Psychiatry*, 96 (January, 1940), 995-997.

³³ F. A. Ross, "Ecology and the Statistical Method," *American Journal of Sociology*, 38 (January, 1933), 507-522.

³⁴ A. J. Jaffe and E. Shanus, "Economic Differentials in the Probability of Insanity," *American Journal of Sociology*, 44 (January, 1935), 534-539.

berg³⁵ on the basis of his statistics of mental disease in New York State thinks there has been an increase beginning with the final decade of the last century. Malzberg concludes that the unwillingness to recognize the increase in mental diseases is due largely "to disputes between eugenists and environmentalists." In making five different assumptions about the future fertility and mortality rates, Tietze shows that the trend of rates till the year 2000 is upward and thus tends to support the Malzberg position.³⁶

With respect to the statistical studies of mental disorder, I am pointing to those counts of the incidence of the various kinds of mental disorder in relation to certain individual attributes such as age, sex, nativity, and race, or in relation to such status conditions as marital, income, education, occupation, religion or mobility. A difficulty arises in making an interpretation of the rates as found, for the findings of such statistical counts are generally used to support one methodological position as over against another.³⁷ Such findings have also been used to advance certain hypotheses which, for the most part, are never tested by collecting and organizing other relevant data.

the reverse that an increase in the number of mentally disordered in the community makes for a pressure on the community to provide more beds—and realistically, most communities in the United States are not likely to provide beds until absolutely necessary to do so.

³⁵ See his *Social and Biological Aspects of Mental Disease*, Chapt. I, Utica, N.Y., State Hospital Press, 1940.

³⁶ Christopher Tietze, "Future Trends of Mental Disease in the United States 1940-2000," in *Trends of Mental Disease*, The American Psychopathological Association, 1945, pp. 11-25.

³⁷ See for example, C. Landis and J. Page, *Modern Society and Mental Disease*, New York, Farrar and Rinehart, 1938; C. Tietze, B. Lemkau, and M. Cooper, "Personal Disorder and Spatial Mobility," *American Journal of Sociology*, 48 (July, 1942), 29-39. Also Rosalind Gould, "Social Factors in Psychopathology," *The Psychologists League Journal*, III (May-August, 1939), 53-58; O. Odegaard, "Emigration and Mental Health," *Mental Hygiene*, XX (October, 1936), 543-556.

It is of some significance to note that the ecological and statistical studies of mental disorders have been carried on apart from the other research developments in the sociological field dealing with similar problems and apparently have had very little influence on these other developments. This is very surprising when one considers that these studies have pointed to various problems which might be studied by more elaborate research techniques. The challenge which they present, however, is seldom accepted.

CULTURE AND PERSONALITY STUDIES

The earlier notion of anthropologists that human personality is somehow a constant variable, the idea represented by Cooley of the universality of human nature, and the conception that human personality is the "subjective aspect of culture" all have been reexamined by the researches of the social anthropologists during the past two decades. The four emphases which have developed from these researches include: (1) the attempt to describe a culture in terms of some dominant theme which runs through the entire society, (2) the relativistic character of personality deviation, (3) the formulation of the idea of basic personality structure moulded by a specific culture, and (4) the reformulation of the psychoanalytic anthropology which had viewed human institutions as largely an extension of the instinctual life of the individual.

Ushering in this era of anthropological research is Margaret Mead's 1928 account of the socialization of the child in Samoa.³⁸ Mead attempted to show that "storm and stress" usually expected to accompany adolescence by our standards was not true for Samoan society and the harmonious and easy adjustment of the Samoan adolescent was related to the nature of the family organization. In later researches, Mead tends to emphasize the same position by pointing to the different cultural definitions attached to the sexual act and play activity in different societies and how these things affect the

³⁸ *Coming of Age in Samoa*, New York: Morrow & Co., 1928.

person's growing up in the society.³⁹

Benedict⁴⁰ in her research attempts to show that contrasting themes labelled by the Apollonian-Dionysian dichotomy run the entire gamut of Zuni and Plains Indian cultures. Then, by examining the extreme paranoia which runs through the Dobu society and the giving away of one's goods as a means of subduing a rival so typical of the Kwakiutl, Benedict is able to show that tendencies and conditions which in our society are regarded as abnormal are essential qualities of the ideal man in the Indian culture of the Northwest Coast. Thus, is portrayed, even though not conclusively, the relativistic character of certain traits which in our society are regarded as abnormal. In other societies, such traits become channellized and institutionalized in the cultural patterns of the society.⁴¹

Another viewpoint in recent anthropological research has been emphasized by Kardiner and his collaborators who have attempted to use psychoanalytic techniques and insights in the analysis of cultural data. In their first volume⁴² the attempt was made to show for two cultures the manner in which the basic personality structure is derived from the primary institutions such as the relation between the sexes, child training devices, subsistence techniques and the like. Thus, the notion of the basic personality structure is presented as a technique for better understanding cultural patterns and institutions. The basic personality structure consequently rests upon the postulate that if

the child training devices are the same in all families of a given culture, the personality organization will not vary between individuals. But, atypical family training, in contrast, is likely to produce some type of deviant personality. The basic personality structure can be regarded as projective into the culture and thus accounting for the various secondary institutions appearing in the culture.

If this account of the basic personality structure can be considered as valid, it was thought that it might be constructed by the study of a given culture *per se*. In his second book,⁴³ Kardiner attempts this through the analysis of the cultural and psychological materials obtained by Du Bois in her study of the Alorese.⁴⁴ By studying such primary institutions as child training practices and certain conditionings for adult life, an attempt was made to show the kind of basic personality structure emerging in the society. The concept of basic personality structure then came to be regarded as a tool for social science research.⁴⁵

The research work of Mead, Benedict, Dollard and Warner in various culture milieus had a certain impact on psychoanalytic psychiatry. Here, Horney⁴⁶ took the lead by viewing the neurosis not as, in the past, a result of repression of libidinal energy or the fixation of the emotional life at some point in its development, but rather in terms of the conflicting demands which a given culture imposes on the individual.⁴⁷ She also attempts to find for our society that the competitive patterns contain the seeds of the

³⁹ See M. Mead, *Sex and Temperament in Three Primitive Societies*, New York: Morrow and Co., 1935. Also see her *Cooperation and Competition Among Primitive Peoples*, New York: McGraw-Hill & Co., 1937.

⁴⁰ See Ruth Benedict, *Patterns of Culture*, New York: Houghton Mifflin Co., 1934 and see also "Anthropology and the Abnormal," *Journal of Genetic Psychology*, 10 (1934), 59-82.

⁴¹ A. I. Hallowell, "The Social Function of Anxiety in a Primitive Society," *American Sociological Review*, VI (December 1941), 889-891; See also his "Psychic Stresses and Cultural Patterns," *American Journal of Psychiatry*, 92, 1291-1310.

⁴² A. Kardiner, *The Individual and His Society*, New York: Columbia University Press, 1939.

⁴³ A. Kardiner, *The Psychological Frontiers of Society*, New York, Columbia University Press, 1945.

⁴⁴ Cora Du Bois, *The People of Alor*, Minneapolis, University of Minnesota Press, 1944.

⁴⁵ See A. Kardiner, "The Concept of the Basic Personality Structure as an Operational Tool in the Social Sciences," in *Science of Man in the World Crisis*, New York, Columbia University Press, 1945, pp. 107-122.

⁴⁶ Horney, although a psychoanalyst, has been included here both because of her influence in shaping the neo-freudian position and because of her influence among sociologists.

⁴⁷ K. Horney, "Culture and Neurosis," *American Sociological Review*, 1 (April, 1936), 221-229.

aggressive strivings and ambivalent hostility which are characteristic of our neuroses.⁴⁸ In a later work, dissatisfied with therapeutic results and certain phases of psychoanalytic theory, she attempts to bring psychoanalysis in line with the newer conceptions in sociology and anthropology and to substitute a "sociological orientation" for an "anatomical-physiological" one.⁴⁹

These culture and personality studies have been suggestive rather than conclusive and it would seem that this position can be maintained with respect to any aspect which one might desire to examine. The conclusion is the same whether one tries to find *a la Kardiner* the exact procedure by which the group culture makes ingression into the personality as "key integrational systems" or whether one tries to get a picture of the relativistic character personality deviations as Mead, Benedict and Hallowell do. With respect to the Kardiner group, it can be pointed out that there is no systematic statement of hypotheses or concepts so that it is frequently difficult to follow them. Then, too, while the basic personality structure may be formed in the childhood years through the operations of the primary institutions, the materials on the various cultures largely illustrate this conception; they do not demonstrate or prove the hypothesis. With respect to the relativistic character of personality deviations, it would seem that the main contribution of these workers is to show that certain behavior forms which have been viewed in our society as psychiatric symptoms are in certain other societies often channeled by the prevailing institutional structures or perform a given function in relation to the total culture. This may be all right as far as it goes but it certainly does not succeed in explaining how these symptoms frequently combine in a person to display a complex deviant mental disturbance which we in our society recognize as one of the functional psychoses. Anxiety or trance-like states which may be functionally useful in certain cultures is one

thing; schizophrenia, one of our diagnostic labels for a functional disorder, is something else again. I am not saying that no attempt should be made to study schizophrenia in cultural terms but am only pointing to the inconclusive and unsatisfactory character of the relativistic personality deviation conceptions.

Then too, the attempt to type cultures in terms of some dominant theme may have a limited usefulness but it has also led to unreasonable applications. Questions appearing during the last two decades such as, "Is Germany suffering from paranoia?" or "Does England have a senile psychosis?" explain nothing but only add to the intellectual confusion of our times.⁵⁰ A conception such as the above would have significance only to the extent that it could be shown that this dominant cultural theme is reflected in the personalities of a large majority of the persons in the society. Even then one is still faced with the problem of who is likely to be regarded as paranoid in a "paranoid" culture.

On the positive side these studies have certainly brought about a working research relationship between psychoanalysis and cultural anthropology. They have stimulated the growth of neo-freudianism with their acceptance of a restatement of the relationship of the individual and society and personality and culture. While this restatement succeeds in incorporating into psychoanalysis the anthropologically empirically derived conception of culture, it has not succeeded in forming a very satisfactory sociological foundation for the emergence of a dynamic psychology of the personality.

CASTE AND CLASS STUDIES

The development of the caste-class framework for purposes of social analysis is peculiarly the work of W. L. Warner,⁵¹ his co-workers and students. True, the class aspect of human society had been portrayed by the

⁴⁸ K. Horney, *The Neurotic Personality of Our Time*, New York: W. W. Norton and Co., 1937.

⁴⁹ K. Horney, *New Ways in Psychoanalysis*, New York, W. W. Norton and Co., 1939.

⁵⁰ Franz Alexander, "Psychoanalysis and Social Disorganization," *American Journal of Sociology*, 42 (May, 1937), 781-813.

⁵¹ W. L. Warner, "American Caste and Class," *American Journal of Sociology*, 42 (September, 1936), 234-237.

first generation of sociologists in the United States but it was not until the fourth decade of the twentieth century that the class concept became a heuristic tool in American Sociology.

The studies which have been reported on by Warner and his students while in most instances not dealing with the socially recognized more abnormal types of human adjustment deserve mention here because of their concern with (1) the nature of human socialization through specific forms of child training (2) the attempt to show the differentiating role of class in the personality structure developed.⁵²

The studies in this group have provided a framework within which personality disorders of our society might be studied. Using his class schema, Warner pointed to three ways of regarding the development of mental disorders.⁵³ He saw these afflictions as a result of (1) changes in social organization produced by technology, (2) the breaking up of institutional symbolic systems, and (3) frustration arising in the person when his vertical mobility was blocked. These three ideas are hardly new and bear a close resemblance to the cultural lag theory, Durkheim's notion of anomie, and Horney's emphasis on the competitive struggle and its role in the genesis of neuroses.

Green, using the class concept, attempts to show the kind of middle class family situation leading to neurosis and the kind of middle class family situation which does not. In the Polish middle class family he finds that the child does not become neurotic because the family does not absorb his personality, while in the American middle class family of several generations the child does become neurotic because as an object of ambivalent parental feelings he is a threat to the social life and career goals of the par-

ents.⁵⁴ Ericson on the basis of interviews with samples of lower class and middle class mothers concludes "that membership in a social class is an important influence on personality development and that there are significant differences in child-rearing practices between social classes."⁵⁵

These studies are pretty much analogous to the personality-culture studies, especially in their emphasis on child training. They, to date, have thrown little light on personality disorders, although they have been quick to show certain abnormal traits, and distorted attitudes as they are nurtured within a social class context. They may eventually prove significant by virtue of calling attention to the class character of some of our personality disorders. Future research utilizing these conceptual tools will produce a more adequate evaluation with respect to clarifying their worth in adding to knowledge of both normal and abnormal personalities.

STUDIES OF INTERPERSONAL RELATIONS

Finally, we come to a consideration of the kind of knowledge which has evolved through the careful scrutiny of various kinds of interpersonal relations and their consequences for personality organization. The studies and theory of this section fall appropriately into the following categories: (1) personality organization, (2) social interactional relationships, (3) behavioral consequences, and (4) sociometric developments.

Personality Organization: This particular area is typified by Dai's descriptive and analytic account of the patient as a person.⁵⁶ True, its results are nothing more than working hypotheses, but it does succeed in pointing to the limitations of biology in dealing with the person and the possible integrations of psychoanalysis and sociology for psychiatry. Most significant is Dai's attempt to

⁵² See A. Davis, and John Dollard, *Children of Bondage*, Washington, D.C., American Council on Education, 1940. See also the other studies of the American Council on Education on the personality development of Negro Youth.

⁵³ W. L. Warner, "The Society, the Individual and his Mental Disorders," *American Journal of Psychiatry*, 94, 275-284.

⁵⁴ A. W. Green, "The Middle Class Child and Neurosis," *American Sociological Review*, 11 (February, 1946), 31-41.

⁵⁵ Martha C. Ericson, "Child Rearing and Social Status," *American Journal of Sociology*, 52 (November, 1946), 190-192.

⁵⁶ Bingham Dai, "The Patient as a Person," in *Social and Psychological Studies in Neuro-Psychiatry in China*, Peking, 1939, pp. 1-30.

classify the possible social etiologies of various personality disorders. In a later paper, Dai attempts to show with reference to the frequencies of various psychoses found among patients admitted to the Peking Municipal Psychopathic Hospital that personality problems are essentially problems of social adjustment and are most easily understood in terms of the analysis of the person's conception of himself.⁵⁷

Dunham's descriptive and analytic account of the social personality of the catatonic⁵⁸ supplements and reinforces Dai's analysis. Here, the trait organization of the catatonic is depicted as being at a variance with the trait organization of the other young men in the community. This situation conditions all of the catatonic's interpersonal relations and consequently he has no way of getting an adequate conception of himself through interaction with others.

Along this same line is Faris' attempt to show the schizophrenic personality as a product of social isolation. Breaks in the communication and socialization processes lead to the trait of seclusiveness which in turn causes the person to withdraw further from society. The final result is the schizophrenic breakdown. Faris' subsequent report of a schizophrenic case implies the process of the building up of a unique world not shared by other persons.⁵⁹

Social Interactional Relationships: Slotkin studied the nature and effects of social interaction among a group of schizophrenics.⁶⁰ His main conclusion was that, since symbolic interaction is largely absent among

schizophrenes because of the private character of the symbolism, then normal symbolic interaction is basic to the development of society and culture.

Rowland has studied the nature of the social process within the mental hospital and the kinds of friendship patterns which develop between patient-employee and patient-patient as a consequence of the interactional process.⁶¹ Dunham and Weinberg in a recent study⁶² have continued these interests showing the kind of cultural organization which develops among patients and employees in a state mental hospital and the impact of these two structures upon the patient, especially as it affects his length of stay in the hospital. Finally, Mangus and Seeley give us a kind of psychiatric *Middletown* in their attempt to discover the results of the abnormal social interactional currents and the twisted interpersonal relations in a rural community.⁶³

Lindesmith depends on the process of communication for the making of drug addicts. He is successful in showing that explanations of drug addicts in terms of psychopathy are inadequate and do not fit the facts. Drug addiction, as Lindesmith demonstrate, is thus social through and through; it evolves within a social process.⁶⁴

Behavioral Consequences: Thorner shows that the pattern of ascetic protestantism accounts for the basic personality structure of the middle class family and subsequently for much of the denial of affection with resulting frustration.⁶⁵

⁵⁷ See Howard Rowland, "Interaction Processes in the State Mental Hospital," *Psychiatry*, 1 (August, 1938), 323-337, also "Friendship Patterns in the State Mental Hospital," *ibid.*, 2 (August, 1939), 363-373.

⁵⁸ H. Warren Dunham and Kirson Weinberg, *Social Psychological Study of a Mental Hospital* (Unpublished Manuscript), 1947.

⁵⁹ See A. R. Mangus and John R. Seeley, *Mental Health Needs in a Rural and Semi-Rural Area of Ohio*, Mimeographed Bulletin No. 195, Department of Rural Economics and Rural Sociology, Ohio State University, Columbus, Ohio, January, 1947.

⁶⁰ See A. R. Lindesmith, "A Sociological Theory of Drug Addiction," *American Journal of Sociology*, 48 (January, 1938), 593-609, also his "The Drug Addict as a Psychopath," *American Sociological Review*, 5 (December, 1940), 914-920.

⁶¹ Isidor Thorner, "Sociological Aspects of Af-

⁵⁷ Bingham Dai, "Personality Problems in Chinese Culture," *American Sociological Review*, 6 (October, 1941), 686-696.

⁵⁸ H. Warren Dunham, "The Social Personality of the Catatonic-Schizophrenic," *The American Journal of Sociology*, XLIX (May, 1944), 508-518.

⁵⁹ See Robert E. L. Faris, "Cultural Isolation and the Schizophrenic Personality," *American Journal of Sociology*, 39 (September 1934), 155-169; also his "Reflections of Social Disorganization in the Behavior of a Schizophrenic Patient," *American Journal of Sociology*, I (September, 1944), 134-141.

⁶⁰ J. S. Slotkin, "The Nature and Effects of Social Interaction in Schizophrenia," *The Journal of Abnormal and Social Psychology*, 37 (June, 1942), 345-368.

Dunham's study of criminal behavior among schizophrenes succeeded in making the differentiation between criminal behavior of so-called normals and criminal behavior of schizophrenics and in pointing to the conditions necessary for criminal behavior to appear in the schizophrenes.⁶⁶

Weinberg is able to show using data of neuroses arising in combat that "neurotic-like reactions may emerge among stable personalities when the experiences are sufficiently critical."⁶⁷ Through analysis of case materials, Taylor finds that apparently normal adjustment in only children is related to the emergence of neurasthenic symptoms.⁶⁸ Davis, attempting to examine by one case the consequence of social isolation, concludes that an absence of speech, inability to walk, no sense of gesture, complete inability to care for self, adds support to the Mead-Cooley theory of socialization. In a final note, Davis concludes that Anna is not the most ideal case for showing the effect of isolation as she was probably deficient from the beginning.⁶⁹ Finally, Devereux offers a hypothetical sociological explanation of schizophrenia by stating that it is a consequence of the failure of a person to make the correct extrapolations from a new environment to which he moves or drifts.⁷⁰

Sociometric Developments: The point of view, researches, and system of therapy developed by Moreno⁷¹ deserve mention here

sectional Frustration," *Psychiatry*, VI (May, 1943), 157-173.

⁶⁶ H. Warren Dunham, "The Schizophrenic and Criminal Behavior," *American Sociological Review*, 4 (June, 1939), 352-366.

⁶⁷ S. Kirson Weinberg, "The Combat Neuroses," *American Journal of Sociology*, XI (March, 1946), 465-478.

⁶⁸ Louis Taylor, "The Social Adjustment of the Only Child," *American Journal of Sociology*, LI (November, 1945), 227-232.

⁶⁹ See Kingsley Davis, "Extreme Social Isolation of a Child," *American Journal of Sociology*, XLV (January, 1940), 554-656, also his "Final Note on a Case of Extreme Isolation," *American Journal of Sociology*, LII (March, 1947), 432-437.

⁷⁰ George Devereux, "A Sociological Theory of Schizophrenia," *Psychoanalytic Review*, 26 (June, 1939), 315-342.

⁷¹ J. L. Moreno, *Who Shall Survive*, Washington, D.C.: Nervous and Mental Disease Publishing Company, 1934.

because of their current contemporary influence in sociological circles.⁷² The point of view represented here is found in the social psychology of Mead and Cooley and is partially developed in psychiatry through the writings of Harry Stack Sullivan. The procedure of charting the frequency and intensity of interpersonal relations in any group is a valuable technique for showing certain aspects of the relations which actually take place in the group. Moreno and his associates have thus provided the student of interpersonal relations with a tool for the study of personality deviation as it appears to emerge in a group context.⁷³

This technique lays an empirical basis for viewing interpersonal relations and serves as a substitute for the descriptions of group life and process which have appeared in the theoretical literature. The therapy of psychodrama, where the person either acts out his inner psychological role alone or with other "auxiliary egos" again throws Mead's role-taking scheme either of the self-other or generalized-other variety on the stage, and the person depicts his conflicts and tensions as they may exist in the reality situation. Thus, this role-taking therapy is a partial demonstration of Mead's theory and also provides the earliest sociograms for charting the interpersonal relations within a situational context. From the stage to the sociometric study of the real life situation is a short step for which Moreno has provided an elaborate scheme of new concepts, such as the tele, aris-totele, social atom and sociometric assignment. Neuroses and maladjustments are products of interpersonal situations arising in group life. The therapeutic scheme developed and the tool for measuring interpersonal rela-

⁷² We have included Moreno in our appraisal even though well aware of the fact that professionally he can be regarded as a psychiatrist rather than a sociologist and our attempt has been to see "social psychiatry" as it has developed in the hands of sociologists. However, we feel justified inasmuch as the sociologists and psychologists have largely supported Moreno's research and therapy. The editorial board of *Sociometry* includes twelve recognized sociologists, about fifty per cent of the total.

⁷³ See his "Interpersonal Therapy and the Psychopathology of Interpersonal Relations," *Sociometry* 1, (1937), 9-76.

tions are devices which should serve to make for a more sophisticated statement of symbolic-interactional psychology.

The studies reported, in this section, while at first glance seem unrelated, can, as we have shown, be classified in a general way. The field is new and largely unexplored from an empirical perspective. These studies and others like them should eventually form the foundation for getting research workers closer to the significant problems, which for the most part, we must learn to formulate. This is perhaps no more clearly demonstrated than in the instance of trying to study the "self." The nature and origin of the self is the core of much social psychology, but this social entity cannot be chiseled out in a definitive fashion until we can develop more satisfactory techniques for observing it develop and watching it change. Such descriptive terms as the distorted self, the guilty self, the wounded self, the projective self, the destructive self and the like are suggestive of the social psychological results evolving from family or other group interpersonal relation, but knowledge about these things will continue to elude us until we can show more conclusively the processes of interpersonal relations which bring about such results. This implies the necessity to develop some better techniques for exploring the psychological character of various kinds of structured relationships from the pair to the complex institution, and to show the manner in which these social entities may be related to the genesis of the self and to the distortion of the self at any stage in its development, fixation or regression. In attacking the problems implied here, it seems not out of place to point out that we need to bring about a fusion of those elements which may prove valid for both symbolic interactional and psychoanalytic psychology.

THE CURRENT SITUATION

In this paper we have, thus, tried to do two things. First, we have attempted to show the dubious character of social psychiatry as a special field of sociology and to point to the emergence of a set of problems centering around the personality problems of man in

society, the answers to which many different disciplines will contribute. Secondly, we have tried to point out and critically evaluate the various kinds of researches carried on by sociologists and anthropologists during the past fifteen years which have been regarded, at least by sociologists, as constituting the field of social psychiatry.

With respect to our first point, we can only wait for time to give the answer, although in accordance with the trends in education and the scientific literature we feel vaguely that we have pointed to what will eventually emerge.

With respect to the research developments during the period under consideration, it seems to the writer that such researches have been far more valuable in evolving new techniques, viewpoints, and hypotheses, and much less significant when it comes to a conclusive demonstration of the exact relationships between personality disorders and some element of interpersonal relations or of cultural situations. We cannot be content continually with offering "supporting" and "inferential" evidence, especially if we continue to adhere to the hypothetical proposition that many personality disorders have their roots in the social milieu.

In a very positive sense all of these studies upon which we have reported have served to bring about a closer working relationship between the various disciplines. In this process, as we have shown, there has been a gradual coming together of certain psychoanalysts, psychiatrists, anthropologists, psychologists, and sociologists in their interests so that gradually they are focusing their attention on certain common problems: the self, cultural influences on the personality, the nature of these influences as personal experience, the differentiation of influences at different societal levels, the nature of interpersonal involvements and cultural or unique character of child training practices—these are the problems which must continue to receive attention if satisfactory answers are forthcoming.

Four specific problem areas need some exploration and may throw light upon some of the generally stated problems above. I

think there is an immediate need for an elaborate follow-up study of patients of different types who are discharged from mental hospitals. We need to know what happens to them, the kind of adjustments they make, their relationships with others and the like. Such a concrete picture might not only be suggestive with respect to etiological theories, but it might also be useful for therapeutic programs. Another problem demanding attention is to explore the nature of contrasting "family psychologies" in our society as well as other societies. Certain families in the interaction of their members succeed in creating psychological atmospheres which have adverse personality consequences.

There is also the need for a sociological study of the mentally disturbed child, the schizophrenic child, and the kind of social situation in which he is nurtured. Such study may focus attention upon the kinds of experience likely to be damaging to the self as it emerges.¹⁴

Finally, it seems to me we need to return to an older emphasis in sociology and repeat many institutional studies by means of the new techniques which have been developed in sociometry, psychoanalysis and social psychology. Such studies should put us in a position of seeing the kinds of interpersonal relations within a given structure to which persons are subjected and give us

¹⁴For an account of such cases, see within a clinical psychiatric framework, see Charles Bradley, *Schizophrenia in Childhood*, New York, The Macmillan Co., 1941.

some valid picture of the psychological atmosphere which is created. In this way we could see how an institutional structure fosters already existing fears, anxieties and insecurities of the person and develops new ones. Such studies should prove valuable not only in showing us the impact of certain interpersonal relations and psychological climates on the person, but also for showing how certain kinds of maladjusted and neurotic persons are frequently permitted to obtain positions of authority and then use their office in the institution to literally inflict their neuroses upon others.

In this area of research endeavor as in other social science areas our big and most significant task is to formulate the correct statement of our problems. This is crucial in all research but as we all know especially difficult in social science research. If we can do this, the techniques for solving these problems can be evolved. When psychiatry as a branch of medicine departs in its research from the traditional clinical biological and physiological orientation it faces the same methodological problems as does sociology. This has been happening, as we have shown, during the last two decades and so in the future, we can expect progress in this area to the extent that significant questions can be raised and that the significance of these questions can be agreed upon by the various scientific disciplines concerned. In this way, we will move toward a meaningful "social psychiatry."

OCCUPATIONAL MOBILITY OF URBAN OCCUPATIONAL STRATA*

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PROBLEM AND METHOD

A LONG established ideal of equality of opportunity is an integral part of the American culture pattern. Our children are early imbued with the faith that regardless of the humility of their beginnings, no doors of opportunity are closed

to them—hard work and talent, the expectation is, will be rewarded. The doctrine is by no means unchallenged, but recent public opinion study has demonstrated that, by and large, most American adults still cling to it. Most believe that one man's opportunities to succeed are as good as another's.¹

* Manuscript received November 24, 1947.

¹Centers, R. "Attitude and Belief in Relation to Occupational Stratifications." *J. Soc. Psy.*, in press.

Though doubtless the belief is supported by a substantial amount of socio-economic mobility as it can be observed by the citizen in the not too infrequent phenomenon of his children or those of his neighbors' bettering themselves in economic pursuits, just how much of such mobility exists is not a question that at present can be answered with any great confidence by the social scientist. Re-

the question, a very prominent one of such being Taussig and Joslyn's *American Business Leaders*, but by far the most comprehensive of such investigations is Davidson and Anderson's *Occupational Mobility in an American Community*.³ Though this latter study is based entirely upon data resulting from a survey of the employed male population of a single small California community

TABLE 1. COMPARATIVE OCCUPATIONAL "INHERITANCE" IN VARIOUS URBAN* OCCUPATIONAL STRATA

Occupational Stratum of Father	N	Per Cent Whose Sons Are of Various Occupational Strata								
		Large Business	Professional	Small Business	White Collar	Farm Owners or Managers	Skilled Manual	Semi-skilled Manual	Farm Tenant or Laborer	Unskilled Manual
Large Business Owners and Managers	16	50	19	6	25	—	—	—	—	—
Professional	41	15	32	17	10	5	12	5	2	2
Small Business Owners and Managers	145	10	11	32	24	3	8	10	1	1
White Collar	77	8	9	10	45	6	9	7	1	5
Skilled Manual	190	6	6	8	17	4	37	20	1	7
Semi-Skilled	127	1	2	6	11	4	22	43	1	10
Unskilled	41	—	6	5	10	4	17	20	1	37

* Though the sampling also included farmers, data for them as fathers are not presented here because the farmers interviewed were sons and often failed to specify, in the cases where their fathers were farmers, whether the father was a farm proprietor, farm manager, tenant or laborer.

search, though its need has frequently been recognized, has failed somehow to be carried out on the large scale and in the requisite detail the problem demands. As long ago as 1927 Sorokin gave emphasis to the importance of such study and reviewed the then existing research on vertical occupational mobility.² Such research had characteristically been based upon small samples typifying only segments of the occupational order and had varied from one study to another in respect to definition and measurement of the relevant variables, and Sorokin did not fail to recognize the essential crudeness of the indices of occupational continuity and to point up the need for further exploration. Several investigations have subsequently added to the accumulating information on

(San Jose) the thoroughness of its execution and the definiteness of its findings establish it as a definitive work and a frame of reference for later study. The results described in this paper can be regarded as in a sense supplementary and confirmatory to certain aspects of it, most specifically the degree of continuity in occupational status between father and son.

The data were collected in connection with a study conducted in the summer of 1945 wherein the occupation of both the subject and that of his father were obtained by person to person interview. Inasmuch as the subjects constituted a nationally representative cross section of the adult white male population rather than a strictly local one, the data constitute a logical follow-up of

² Sorokin, P. *Social Mobility*, 1927.

³ Stanford University Press, 1937.

Davidson and Anderson's work and provide an objective check as to the generality of its findings. In general, the results, where comparisons are possible, show substantially the same trends as their study disclosed.

OCCUPATIONAL MOBILITY AND CONTINUITY AS VIEWED FROM THE FATHER'S STATUS

In Table 1 are indicated some relations of fathers' and sons' occupational positions as viewed from the position of the father. In the table is shown the percentages of fathers in each of several occupational categories having sons of various occupations. Only one son of a given father is shown by this table, since sons rather than fathers constituted the actual respondents, but it is an easy and reasonable assumption that the sons interviewed can be taken as an unbiased sampling of the total number of sons of fathers in the various occupational categories, since they were selected quite at random as far as their relationship to the father is concerned. If these data reflect the true facts, then it is clear that by far the most obvious relationship to be found in Table 1 is the tendency for the occupational level of the son to be substantially more commonly the same as that of his father than to be of any other particular level. For example, large business owners and managers most commonly had sons who were also large business owners or managers, and, similarly, professional men more commonly had sons who also became professional men than they had sons who became small businessmen or white collar workers or whatnot. The correspondence of levels is most distinctive of all in the case of white collar workers, for 45 per cent of such men had sons who also became white collar workers of one sort or another.

It cannot be said, despite this high degree of correspondence, that the *majority* of fathers have sons who follow them in being placed in the same occupational stratum, for just the contrary is the actual state of affairs. While it is more common to find fathers whose son's occupational level is the same as their own than it is to find sons located in any one other of the several distinctive strata, it is still true that more sons are to

be found in occupational strata other than the fathers' than are found in it. In the main both these relations are also exhibited by Davidson and Anderson's data.⁴

It may be objected, possibly, that use of the criterion of identity of stratum placement of father and son as an index to the amount of occupational inheritance is too strict a delimitation of the phenomenon, that more such inheritance than non-inheritance might be the case if broader and looser boundaries of occupational categories were employed. In answer, it should be pointed out that the occupational categories utilized (based on Edwards' Census classification) are themselves each rather broad and that each is comprised of numerous sub-groups of roughly similar status and character, so that they do not indicate precisely identical occupation of father and son. If father and son are both skilled manual workers they would, in terms of these groupings, represent a case of occupational continuity between father and son, but such continuity would still not be exact, for the father might be a carpenter whereas the son is a bricklayer. Obviously the amount or degree of occupational inheritance depends in part upon what conception of it is employed. The kind of data being used here give occupational continuity only in terms of similarity of occupational stratum, but if continuity of occupation is to represent a condition of relative social *immobility*, as it often is, in contrast to social *mobility*, then use of status levels such as Edwards' is probably more desirable than use of certain finer categories would be. If a carpenter's son becomes a plumber there is not strict occupational continuity, but neither is there appreciable social mobility, since the son has merely entered a different occupational category at the same level of skill as that of his father and enjoys roughly the same social and economic status as the father relative to other occupational strata. Continuity of essential status is probably the most useful and reasonable concept of occupational inheritance.

⁴ Cf. Table 4 and page 23 of their *Occupational Mobility*.

If one broadens the occupational categories somewhat to take into the considerations of similarity of son's occupation to father's the immediately adjacent levels both above and below a given occupational stratum the data show even more clearly that there is a substantial amount of continuity of occupational status from father to son—that such continuity is indeed, the rule (Table 2). Considering the urban population as a whole, 71 per cent of fathers had sons whose present placement is at their own or an immediately adjacent level.⁵ The percentage of continuity varies considerably among the occupational categories listed, ranging from a high of 76 per cent relative continuity for the semi-skilled to a low of 58 per cent for the unskilled. The skilled manual, white collar, business and profes-

some level of manual labor, if we add the 17 per cent of those whose sons are found in the skilled stratum.

Still other aspects of socio-economic mobility and immobility can be shown by these data by other systems of grouping. For example, one may use only two broad categories, placing all business, professional, white collar and farm owners and managers in one group, with all manual workers and farm tenants in another, and measure the amount of mobility across these boundaries. The data of this study show a rather high degree of continuity within these two strata from father to son. Eighty per cent of fathers in the first group had sons who remain in that group. Also, two-thirds (or 67%) of fathers in the second group have sons in that group now. By this treatment there is ob-

TABLE 2. PERCENTAGES OF FATHERS IN VARIOUS URBAN OCCUPATIONAL STRATA HAVING SONS WHOSE OCCUPATIONAL LEVEL IS RELATIVELY SIMILAR TO THEIR OWN

Occupational Stratum of Father	Per Cent	Categories Included as Relatively Similar in Each Case
Large Business	69	Large Business and Professional
Professional	64	Large Business, Professional and Small Business
Small Business	70	Professional, Small Business, White Collar and Farm Owners and Managers
White Collar	70	Small Business, White Collar, Farm Owners and Managers and Skilled Manual
Skilled Manual	72	White Collar, Farm Owners and Managers, Skilled and Semi-Skilled
Semi-Skilled	76	Skilled, Semi-Skilled, Unskilled and Farm Tenants and Laborers
Unskilled	58	Semi-Skilled, Unskilled and Farm Tenants and Laborers
All Strata	71	

sional groups show varying degrees of continuity lying between these extremes. The unskilled, though manifesting the least continuity in terms of the criterion used, do not, it should be noted, have sons whose occupational status differs in any really great respect from their own, since 75 per cent of unskilled have sons whose occupation is at

viously a modest net mobility in the upward direction, suggesting that the chances of one's son rising in the world are somewhat better than those of his falling. If this is the actual fact it may possibly be accounted for as a result of the relative increase in number of white collar jobs that has been taking place over a period of several decades and the tendency for the birth rate of white collar occupational groups to be lower than that of manual workers. It is known, for example, that certain groups such as professional people do not even reproduce themselves.⁶

Perhaps a more accurate measure of net

⁵ Because the occupational station of Farm Owners and Managers is such a broad grouping it is difficult to locate it in any precise way on a scale of levels composed mainly of urban groups. Since it is a better occupational level than many white collar workers occupy, but poorer than that occupied by many skilled workers, its overlapping with these makes it desirable to include it where they function as adjacent levels.

⁶ Britt, S. H. *The Social Psychology of Modern Life*, Rinehart, N.Y., 1941, p. 329.

mobility would take account, not only of movement into or out of such broad strata as these but movement of sons in either direction, up or down, from the fathers' own position. In Table 3 this aspect of mobility is shown. In terms of the total population of fathers in the sample, the strongest tendency is for them to have sons who remain in the

non-manual ones. This way of treating the data also makes it impossible for an extreme group such as large businessmen to manifest any upward mobility of sons, since there is on this scale no higher group in which to go. But neither, of course, is there any such group in the social order, and the only kind of upward mobility that could occur with

TABLE 3. NET OCCUPATIONAL MOBILITY OF SONS OF PERSONS IN VARIOUS OCCUPATIONAL STRATA

Occupational Stratum of Fathers	Percentages Having Sons Whose Occupational Status Is:			Net Mobility of Sons in Per Cent and Direction
	Better Than Their Own	The Same as Their Own	Not as Good as Their Own	
Large Business	—	50	50	50 downward
Professional	15	32	53	38 downward
Small Business	21	32	47	26 downward
White Collar	27	45	28	1 downward
Skilled Manual	41	31	28	13 upward
Semi-Skilled	46	43	11	35 upward
Unskilled	63	37	—	63 upward
All Strata	35	36	29	6 upward

same occupational stratum they themselves occupy, but this accounts for only somewhat over one-third of the cases (36%). Nearly as many fathers have sons who better their position to some degree or other (35%), while the substantial, and almost as large, remainder (29%) have sons whose positions are inferior to their own. If the percentage representing downward movement is subtracted from that representative of upward, only an over-all net mobility of 6 per cent remains.

When mobility and immobility are considered from the standpoint of various specific occupational strata some interesting differences are found, with the outstanding relationship displayed being the tendency for net mobility (as an algebraic resultant of upward as against downward movement) to be least, in fact almost zero, at the center of the occupational hierarchy and to increase progressively toward the two extremes, being increasing downward net mobility for the upper occupational strata and increasing upward net mobility for lower and lower occupational levels.

Doubtless this is still an imperfect means of computing net mobility, since the strata are themselves rather broad, especially the

sons of such fathers would be betterment of their status within this level. Doubtless a part of those in this large business category listed as having sons whose positions are the same as theirs represent cases where this has occurred, so the data imperfectly reflect the precise facts in that respect. A similar limitation on mobility obtains in the case of the unskilled manual stratum, for sons begotten in this stratum can not go into occupations that are lower than their fathers', since no lower stratum exists.

The findings as they have been considered so far tend to lend reality to the claim that America is a land where extensive upward mobility can and does occur, since somewhat over a third of urban fathers beget sons who achieve higher occupational status than they themselves enjoy, but it is probably only a person given to looking only at the rosy side of phenomena who could feel any particular elation over the fact. It is equally as correct, but does not make one feel as good, to say that nearly two thirds of white American males have sons whose occupational status is either the same as or no better than their own.

So far, no account has been taken of an-

other significant aspect of mobility; namely the *extent* of movement up or down the occupational hierarchy of the offspring of fathers in various positions. Obviously, for example, offspring of parents whose occupational status is semi-skilled labor manifest varying extents of movement in their separate careers, since some go higher than others. Certain such sons may go only one step above the parent into the skilled stratum, while others may go several steps above, even becoming business or professional men.

Table 1 shows that sons of fathers in a given occupational stratum may be distributed over the entire occupational range, and this is in fact, typically the case. There is an orderliness in the distribution, however, and typically fewer and fewer fathers of a given stratum have sons going into occupations other than their own as occupations recede in the occupational order from their own. Taking the occupation of the father as the point of reference in each case, it is typical for the son not to have differed in occupational placement very far in either direction.

At least a crude index of the average "distance" of movement of sons may be had here, moreover, by considering each occupational group as a step on a scale and assigning a score for each son in terms of how many steps away from his father's occupational status in either direction his position is. For example, taking sons of white collar workers: small businessmen whose fathers were white collar are one step (and one score point) above the father, professional men are two steps above, skilled workers are two steps below, etc. Table 4 shows for each occupational stratum the average number of steps away from the father's position, either up or down, the sons of persons of various occupational strata have moved, and also the average number of steps sons of the total sample of fathers have moved. This latter figure, which may be taken as the over-all net *distance* mobility, shows that, on the average, sons have occupations only .35 of a step away from and above the occupation stratum of the father. Not only then, is the over-all net mobility small in amount (Table 3) but it is also small in extent. Also, just as the

TABLE 4. AVERAGE DISTANCE OF OCCUPATIONAL MOVEMENT OF SONS AND PREVAILING DIRECTION FROM FATHERS' POSITION

Occupational Stratum of Father	Average Steps Away of Sons' Occupational Positions*
Large Business	-2.13
Professional	-2.07
Small Business	-1.10
White Collar	-.38
Skilled Manual	+1.08
Semi-Skilled	+1.42
Unskilled	+3.77
All Strata	+ .35

* A plus sign indicates average steps above the fathers' position. A minus sign indicates average steps below the fathers' position.

amount of mobility (as shown by Table 3) is greatest at the extremes of the occupational scale and least at its center, so is the *distance* of movement greatest at the extremes and least at the center of the scale.

OCCUPATIONAL CONTINUITY AND MOBILITY AS VIEWED FROM THE SONS' STATUS

In order to get a clear understanding of the degree of correspondence between the occupational status of fathers and that of sons Table 5, which gives a backward view from occupational status of sons to that of their fathers, has been prepared. The sons were the actual respondents in this study and comprise a cross-section of the urban adult white male working population as it is now constituted. Table 5 indicates what percentage of sons in a given occupational grouping are drawn from fathers in the various occupational levels. It tells us, as Table 1 does not, from just which strata those who make up today's working force have come. It is shown, for example, by line three of Table 1 that of the 145 fathers listed as small business owners or managers 32 per cent, or 46 of them, had sons who also became small businessmen. But when the third line of Table 5 is examined, it is shown that these 46 small businessmen sons of small businessmen fathers constitute 53 per cent of today's small businessmen. In other words, even

TABLE 5. OCCUPATIONAL ORIGINS OF A SAMPLE OF TODAY'S URBAN LABOR FORCE

Occupational Strata of Son	N	Percentages Whose Fathers Are of Various Occupational Strata						
		Large Busi- ness	Profes- sional	Small Busi- ness	White Collar	Skilled Manual	Semi- skilled Manual	Un- skilled Manual
Large Business Owners & Managers	46	18	13	30	13	24	1	—
Professional	57	5	23	28	13	21	5	5
Small Business Owners & Managers	87	2	8	53	9	18	8	2
White Collar	129	3	3	27	27	26	11	3
Skilled Manual	116	—	4	10	6	50	24	6
Semi-Skilled	122	—	2	11	4	31	45	7
Unskilled	48	—	2	2	8	29	28	31

though only about a third of the last generation's small business owners and managers had sons who were also small businessmen, these constitute over half of today's small businessmen. Somewhat the reverse situation obtains with respect to professional men. Whereas by Table 1 it is shown that 32 per cent of professional fathers had professional sons, Table 5 indicates that only 23 per cent of today's professional men were drawn from professional fathers.

Table 5 indicates also that sons in a given occupational stratum tend to be drawn most heavily from fathers in the same or a closely contiguous stratum (inspect also Table 6), but that sons are also characteristically drawn more heavily from certain strata than others. For example, whereas substantial proportions of sons in nearly every stratum had fathers whose occupation was skilled labor, comparatively small percentages of sons in other than the unskilled stratum had fathers who were unskilled, and likewise few sons in any stratum had fathers in either the

large business, the professional or the white collar strata. The determinants of these re-

TABLE 6. PERCENTAGES OF SONS IN VARIOUS URBAN OCCUPATIONAL STRATA DRAWN FROM FATHERS OF THE SAME STRATUM OR AN ADJACENT STRATUM

Occupational Stratum of Son	Per Cent
Large Business Owners & Managers	31
Professional	56
Small Business Owners & Managers	70
White Collar	80
Skilled Manual	80
Semi-Skilled Manual	83
Unskilled Manual	59

lations can only be guessed at, of course, but the two main factors would probably be found in the changing requirements of the productive economy for workers at various levels, and in the differing numbers supplied to the population by the fathers of the various occupational strata, and from which the labor force can be ultimately drawn.

OFFICIAL REPORTS *and* PROCEEDINGS



ANNOUNCEMENT OF CHANGE IN EDITORSHIP OF THE REVIEW

Maurice R. Davie, Yale University, will become the Editor of the *Review* beginning with the October issue. Manuscripts submitted for publication in the *Review* should be sent to Dr. Davie after May 1. Books submitted for review should be sent to Dr. Davie after July 1.

Dr. Angell will continue to edit the *Review* to and including the August issue and manuscripts may be sent to him until May 1.

Dr. Davie has selected as his Book Review Editor: Stephen W. Reed, 133 University Hall of Graduate Studies, Yale University, New Haven, Conn.

PROCEEDINGS OF THE SOCIETY AND THE EXECUTIVE COMMITTEE

First Meeting of the Executive Committee on December 27, 1947

The Executive Committee of the Society met with Franklin Frazier, Kimball Young, Florian Znaniecki, Katharine Jocher, Robert Lynd, Robert LaPiere, Carl Taylor, Harold Kaufman, present, and President Louis Wirth presiding.

The following actions were taken by the Executive Committee:

James H. S. Bossard was re-elected a member of the Classification Committee and designated as the Chairman.

The revised report of the Committee on Relationships with Sociologists in Other Countries was approved.

The action of the President in appointing a Committee on Sociological Articles in the Scientific Monthly was ratified and the present committee continued.

Approval was voted of a Resolutions Committee consisting of Franklin Frazier (Chairman), Carl Taylor, and Robert Lynd.

A motion was made and approved that the Executive Committee recommend that for the annual meeting of 1948 the Program Committee be instructed to formulate the program by calling upon the members of the American Sociological Society to submit in advance proposed papers or abstracts of same, from which papers, as far as possible, the final program be made up. To facilitate this selection, the Program Committee is further authorized to appoint a Committee on Contributed Papers,

the Program Committee, however, to have final decision.

It was voted to amend Section 9, Article 1, of the By-laws to read as follows: Joint memberships in the categories of active and associate memberships may be taken out by husband and wife upon payment of dues of seven dollars per annum, both of whom individually shall have all the rights and privileges to which their respective categories of membership in the Society entitle them, provided that they shall together be entitled to one subscription to the *Review*.

Re-election of Ernest R. Mowrer as Secretary-Treasurer.

Empowerment of the Secretary to reclassify members eligible for emeritus membership upon the receipt of the appropriate information.

Election of Frank H. Hankins and E. W. Burgess to the Committee on Budget and Investments.

Appointment of Irene Taeuber to continue to represent the Society on the Dewey Decimal Classification Committee.

The meeting adjourned at 6 P.M. and was reconvened at 9 A.M. on December 28, with the following persons present: Franklin Frazier, Raymond V. Bowers, Florian Znaniecki, Harold Kaufman, Carl Taylor, Kimball Young, Katharine Jocher, S. A. Stouffer, Robert Lynd, Robert C. Angell, and Noel P. Gist; President Louis Wirth presiding.

The following further actions were taken by the Committee:

Inclusion in the Budget of the sum of \$300 for miscellaneous committee expenditures to be expended at the discretion of the Administration Committee, and if more is needed this sum to be augmented by an additional amount at the discretion of that Committee.

Approval of the annual budget of the Secretary and Managing Editor.

Election of Kimball Young to represent the Society on the Social Science Research Council.

Election of Frank Lorimer to succeed Irene Taeuber as the Society's representative to the American Documentation Institute.

Approval retroactively of the action of the President in supporting the American Historical Association's Committee on Documentary Reproduction in the effort to obtain microfilm

materials of value under the Fulbright Act.

Acceptance of the report of the Committee on Education in Sociology with thanks for its thorough and constructive survey.

Approval of the continued publication of the membership list.

Election of Louis Wirth as member of the Administration Committee.

Authorization of the Secretary to sign the new contract with the Banta Publishing Company for the publication of the *Review*.

The Executive Committee examined the questionnaire returns with regard to a meeting on the Pacific Coast, and, finding that less than one hundred members not living in the Pacific coast area would attend an annual meeting held upon the Pacific coast, decided that it would be advisable, therefore, to hold the next annual meeting in the middle west, and set the time between Christmas and New Year's.

The Executive Committee considered the vote of the Society at its 1946 meeting requesting the officers of the Society to submit suggestions whereby the interests of the members in locating vacancies and in teachers' salaries could best be served. The Committee does not see any way in which the Society can effectively influence the salary scales of teachers. In view of recent trends toward specialization, and in view of the complicated nature of the problem and the limited resources of the Society, the Executive Committee recommends to the Society that representation be made to the Social Science Research Council to make available to the Society the experience of graduate departments of Sociology and other societies with placement problems, and a summary report on how graduate students are inducted into teaching and other professional careers with a view to considering whether a personnel and placement service might be made available to sociologists and other social scientists.

The meeting adjourned at 12 o'clock and was re-convened at 9 A.M. on December 29, with the following persons present: Raymond V. Bowers, Carl Taylor, Noel P. Gist, Franklin Frazier, Katharine Jocher, Robert Merton, and Robert Angell; President Louis Wirth presiding.

The following further actions were taken by the Committee:

Election of George A. Lundberg as representative of the Society to the American Association for the Advancement of Science.

Election of Thorsten Sellin to represent the Society to the American Prison Association.

The Executive Committee voted: "Be it resolved by the Society that it co-sponsor the

centennial meeting of the A.A.A.S. in September 1948, but that due to previous plans the annual meeting of the Society cannot be held jointly with the A.A.A.S. at that time. However, the members of the Society are encouraged to support and participate in the centennial meeting of the A.A.A.S."

Acceptance of the progress report of the Committee on Monographs and agreement that such a monograph series is a desirable undertaking. However, since the demand for such a service is not known, it is recommended that the demand be further investigated, that sources of funds be canvassed and that the present Committee be continued for another year to report at the next annual meeting. Appreciation was expressed for the work of the Committee this year.

Acceptance of the Research Committee report and resolved that the members of the Committee serve for 3-year terms, one-third to be appointed each year. It was further resolved that the Society finance at least one meeting of the Research Committee each year. Third, resolved that the Research Committee's recommendation for the establishment of a new research journal be referred to the Committee on Monographs.

Resolved that the Secretary of the Society be authorized to participate with the secretaries of other social science societies with reference to joint meetings and common programs.

The Executive Committee voted to re-elect Robert C. Angell, Editor of the *Review*, and requested that the Committee be empowered by the Society to select the next Editor of the *Review* before the next meeting of the Society.

Approval of the report of the representatives of the Society on the S.S.R.C.

Recommended that the incoming administration appoint an ad hoc Committee to study the matter of a paid secretariat for the Society.

Respectfully submitted,

ERNEST R. MOWRER, *Secretary*

Second Meeting of the Executive Committee on December 30, 1947

The Executive Committee of the Society met with Franklin Frazier, Noel Gist, Robert Angell, Carl Taylor and Katharine Jocher, present, and President Louis Wirth presiding.

The following actions were taken by the Executive Committee:

Received and approved the report of the Committee on Statistics which reads as follows:

The Social Statistics Committee presents two resolutions. These arise because of the dif-

difficulties which it is anticipated that the federal agencies conducting statistical functions will face in obtaining adequate funds for their program. Specific recommendations are formulated on only two matters which have been called to the attention of the Committee. The Committee emphasizes the fact that action by the Society as an organization may have less weight than the individual action of an important proportion of its members, so that the passing of these resolutions does not relieve each member from the responsibility of expressing himself directly.

The specific resolutions are as follows:

1. Congress will be asked in the near future to provide funds for preparatory work for the 1950 census and a few months later, to provide funds for the taking of this Census. In view of the great importance of Census data from a wide variety of standpoints, the officers of the Society are requested, at the proper time, to urge the members of the Appropriations Committee and other Congressmen to provide adequate funds for these undertakings. Because of rising costs, much larger appropriations will be needed than in 1940 to provide for a Census as adequate as that of 1940, to say nothing of the provision for the improvements which are needed.

2. The United States has lagged far behind most Western nations in collecting information regarding marriage and divorce. Attempts by government agencies to correct this situation have been severely impeded by the failure of Congress to provide adequate funds. Therefore, the officers of the Society are requested at the proper time to urge the members of the Appropriations Committee and other Congressmen to appropriate the money necessary for these undertakings.

Recommended that in view of special interests of sociologists in Latin American studies and relations, the Committee on Relations With Sociologists in Other Countries give consideration to the interests of this group.

Recommended that the Society be authorized to arrange for facilities whereby institutions seeking staff, and personnel seeking placement can be served at the next annual meeting.

Approval of the reports of the Resolutions Committee which read as follows:

"Be it resolved that the Society express its appreciation to Dr. Ernest Mowrer, the Secretary-Treasurer, and Dr. Robert Angell, the Editor of the *Review*, for their efficient and loyal services to the Society."

"Be it resolved that the Society express its thanks and appreciation to Professor W. C. Waterman and his Committee on Local Arrangements for their part in facilitating the success of the Annual Meeting."

"Be it resolved that the Society express its thanks and appreciation to the Hotel Commodore for its accommodations and courtesies during the Forty-second Annual Meeting of the Society."

Respectfully Submitted,
ERNEST R. MOWRER, *Secretary*

First Business Meeting of The American Sociological Society, December 29, 1947

President Wirth in the chair.

President Wirth called upon Kimball Young to take the floor, whereupon Mr. Young, Chairman of the Committee on Memorial concerning the late William I. Thomas, read the following statement and then moved that it be spread on the minutes of the Society, and that a copy be sent to Dr. Thomas' survivors. The motion was seconded and carried by a unanimous standing vote:

The members of the American Sociological Society deeply regret the passing of one of its founders, William I. Thomas. His contributions to the advancement of sociology were of high order. To mention only some of the most significant: He was a pioneer in the field of race relations where he always stressed the need of empirical research before effective action programs could be expected. He was among the very first to bring about a real linkage between cultural anthropology and sociology, as evidenced in his *Source Book for Social Origins*, 1909. His *Polish Peasant in Europe and America*, 1918-1920, was, and is, a high water mark in the description and analysis of acculturation. He contributed much to the development of social psychology, both in matters of theory and in empirical research. His use of the concepts of the four wishes, of attitude and value, and later his situational approach, all helped extend the frontiers of our knowledge about social behavior.

Dr. Thomas was a most stimulating teacher and always a friendly and helpful critic of the work of others. As a sociologist, he continually stressed the need of maintaining close relations between research and theory. As a person he was warm and outgoing, and one whose zest for life was contagious.

The chair announced the membership of the

Resolutions Committee consisting of Franklin Frazier, Carl Taylor, and Robert Lynd.

Reports of the Secretary, Treasurer, and Managing-Editor were heard and it was moved that they be received and placed on file.

Kimball Young reported upon the work of the Social Science Research Council of which he is one of the Society's representatives. A summarization of the main features of the report were presented, the complete report to be published later in the *Review*.

Talcott Parsons, the Society's representative to the American Association for the Advancement of Science, reported upon the relations of the Society with the A.A.A.S., particularly upon the work of the inter-society committee in its efforts to draw up legislation relative to the establishment of a Science Foundation.

The Secretary, who served as one of the Society's representatives, reported on the annual meeting of the American Council of Learned Societies at which constitutional changes were made.

It was moved that the reports of the representatives to the S.S.R.C., A.A.A.S. and A.C.L.S. be received and accepted. This motion was passed.

The Secretary read the minutes of the first meeting of the Executive Committee. It was moved to approve these minutes and voted to accept them except for the amendment to the By-laws concerning joint memberships which, in accordance with the constitution, was laid on the table to be considered at the second business meeting.

The meeting adjourned.

Respectfully submitted,

ERNEST R. MOWRER, *Secretary*

Second Business Meeting of the American Sociological Society, December 30, 1947

President Louis Wirth in the chair.

The minutes of the preceding meeting were approved as read.

The minutes of the second meeting of the Executive Committee were read and approved.

The Amendment to the By-Laws of the Society which, in accordance with the requirements of the Constitution, had been laid upon the table at the previous business meeting, were taken from the table and approved.

The Chairman of the Committee on Nominations, Raymond Kennedy, was called upon to read the names of his Committee which consists of: Ray E. Baber, A. J. Jaffe, Edgar T. Thompson, George B. Vold, Raymond V. Bowlers, Margaret Hagood, Harry Alpert, Harold

W. Saunders, Lloyd A. Cook, Noel P. Gist, Arnold Rose, Calvin F. Schmid, Austin L. Porterfield, C. Horace Hamilton.

It was moved and voted that the meeting adjourn.

Respectfully submitted,

ERNEST R. MOWRER, *Secretary*

ANNUAL REPORT OF THE SECRETARY

FOR THE FISCAL YEAR, DECEMBER 1, 1946

TO NOVEMBER 30, 1947

Membership in the Society has increased over last year as the following comparative figures show:

Type of Membership	1946	1947
Active	1,202	1,370
Associate	—	111
Student	300	456
Joint	62	66
Donor	7	1
Life	31	33
Honorary	5	5
Emeritus	13	15
Free Armed Forces	28	—
Exchange	3	—
Total	1,651	2,057

The Society is particularly indebted to Professor Delbert Miller and his Committee on Membership for a good share of the increase in membership. This Committee was responsible for recruiting 259 of the new members. Other members of the Society were also especially helpful in the membership drive having recruited 253 new members, bringing the total to 512 new members.

The Society lost through death the following members: Clarence Case, Charles Galpin, William I. Thomas.

Respectfully submitted,

ERNEST R. MOWRER, *Secretary*

ANNUAL REPORT OF THE MANAGING EDITOR

FOR THE FISCAL YEAR, DECEMBER 1, 1946

TO NOVEMBER 30, 1947

The year 1947 has been one both of increased income and expenditures so far as the *Review* is concerned. Circulation of the *Review* is higher than ever before, the number of copies of the December issue being well over 3,200. In part this is the result of increased membership, but subscriptions have also increased substantially.

From the standpoint of income, subscriptions yielded \$3,045 in comparison to \$2,891 in 1946;

sales of back issues of the *Review* brought in \$1,684, in comparison with \$314 the preceding year. This increase is almost wholly the result of the special sale of the earlier volumes which was authorized by the Executive Committee at last year's meeting. Income from advertising has increased from \$1,316 in 1946 to \$1,599 in 1947. In part this increase represents increased advertising rates which went into effect upon July 1 but it is mostly a reflection of increased sale of advertising space.

Expenditures in connection with the publication of the *Review* have also increased. The most substantial part of this increase has resulted from the increased cost of printing. The coming year's costs will be further augmented under the new contract with the Banta Publishing Company which calls for an increase of 24 per cent over last year's contract price. The prospects are, however, that the *Review* for 1948 will have as many pages as last year if income continues to keep pace with expenditures.

Respectfully submitted,
ERNEST R. MOWRER,
Managing-Editor

ANNUAL REPORT OF THE TREASURER

FOR THE FISCAL YEAR, DECEMBER 1, 1946
TO NOVEMBER 30, 1947

Financially, the year 1947 has shown considerable improvement over the previous year. Total receipts amounted to \$17,875 in comparison to \$13,155 in 1946. Total expenditures also increased from \$10,873 in 1946 to \$14,157. Excess of income over expenditures for the fiscal year was \$3,727 as compared with \$2,282 the preceding year.

Nevertheless, the Society is confronted with the trend of rising costs. Costs of clerical help have increased considerably, and were it not so difficult to find competent help this item in expenditures would have been greater than it was. Curtailed services to the membership have of necessity developed out of this shortage. Printing costs have substantially advanced as have most of the other items. Increased membership has augmented the receipts from dues and thus made it possible to continue to operate without increasing the dues.

Details of income and expenditures for the past fiscal year are covered in the Auditor's Report on file in the office of the Treasurer and to be published in the April issue of the *Review*.

Respectfully submitted,
ERNEST R. MOWRER, *Treasurer*

AUDITOR'S REPORT

FOR THE FISCAL YEAR, ENDED
NOVEMBER 30, 1947

December 24, 1947

Executive Committee
American Sociological Society
1822 Sheridan Road
Evanston, Illinois

Dear Sirs:

In accordance with instructions we have examined the available records of the American Sociological Society for the fiscal year ended November 30, 1947. We submit herewith the following exhibits:

Particulars	Exhibit
Statement of Cash Receipts and Disbursements—Fiscal Year Ended November 30, 1947	1
Inventory of Securities—November 30, 1947	2

SCOPE OF DETAIL VERIFICATION

The extent of our verification is outlined below:

- (1) Reconciled all cash receipts and disbursements as shown by cash journal with deposits and withdrawals on bank statements.
- (2) Reconciled balance in checking account with confirmation received directly from the State Bank and Trust Company, Evanston, Illinois.
- (3) Compared all checks cleared by bank with entries in cash journal for payee and amount; examined checks for signature, endorsements and bank perforation.
- (4) Accounted for consecutive numbers of checks drawn on State Bank and Trust Company, Evanston, Illinois.
- (5) Accounted for interest and dividends due on securities owned and traced receipt to the checking account.
- (6) Refooted cash receipts and cash disbursements records.
- (7) Examined securities at the vaults of the State Bank and Trust Company, Evanston, Illinois, on December 19, 1947.

The present Accounting System is limited to accounting for cash receipts and disbursements; there is no journal or ledger. Hence the usual income statement and balance sheet were omitted. Recommendations for improvements will be submitted in a supplemental report.

We express our appreciation of the courtesies extended our representatives during the course of their work.

Very truly yours,
DAVID HIMMELBLAU & Co.
Certified Public Accountants

OFFICIAL REPORTS AND PROCEEDINGS

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Exhibit 1

STATEMENT OF CASH RECEIPTS AND DISBURSEMENTS FOR FISCAL YEAR ENDED NOVEMBER 30, 1947

Receipts

<i>Dues—</i>		
Active or associate membership	\$8,055.00	
Student membership	1,618.50	
Joint membership	231.00	
Life membership	415.00	
Donor membership	60.00	\$10,379.50
Subscriptions to <i>Review</i> (net)		3,045.90
<i>Sales of publications—</i>		
<i>Review</i>	\$1,684.20	
<i>Papers and Proceedings</i>	176.80	1,861.00
Advertising in <i>Review</i>		1,599.12
Dividends received on investments		233.21
Registration fees—1946 annual meeting		601.50
Royalty income		36.95
Miscellaneous receipts		118.38
Collections on subscriptions to other magazines to be entered for members		1,865.45
Total		<u>\$19,741.01</u>

Disbursements

Cost of printing and mailing <i>Review</i>	\$ 8,762.24	
Clerical aid to Secretary, Editor and Managing Editor	2,608.79	
<i>Committee expense—</i>		
Census of research	\$140.85	
Membership	131.57	
Nominating	193.10	
Public relations	100.00	565.52
Postage, telephone and telegraph expense		309.04
Printing and stationery expense		640.05
Office expense		220.53
Annual meeting expense		483.52
Expense of moving office to Evanston, Illinois		138.81
Audit expense—1946		150.00
Advertising expense		135.00
Refunds		80.80
Miscellaneous expense		63.67
Payments for subscriptions to other magazines entered for members		1,871.50
Total		<u>\$16,029.47</u>

Summary

Balance December 1, 1946	\$ 6,543.34	
Receipts	19,741.01	
		\$26,284.35
Disbursements		16,029.47
Balance November 30, 1947 (State Bank and Trust Company, Evanston, Illinois)		<u>\$10,254.88</u>

INVENTORY OF SECURITIES—NOVEMBER 30, 1947

		Par Value or Number of Shares	As Reported in 12-30-46 Report	Dividends Received
Description	Type			
Bonds:				
United States Savings Bonds	Series F, due 6-1-57	\$2,000.00	\$1,480.00	\$ -
Hyde Park Baptist Church of Chicago	4% Bearer Bonds, due 5-1-53 (May, November, 1947 and subsequent coupons attached)	600.00	600.00	-
Stocks:				
American Telephone and Telegraph Company	Common stock, \$100 par	3	296.00	27.00
United States Steel Corpora- tion	7% Cumulative preferred stock, \$100 par	5	532.41	43.75
Standard Oil Company of New Jersey	Common stock, \$25 par	12	533.56	42.96
West Penn Electric Company	7% Cumulative preferred stock, \$100 par	2	185.18	14.00
Consolidated Natural Gas Co. of Delaware	Common stock, \$15 par	1	15.00	2.00
Union Pacific Railroad Company	Common stock, \$100 par	10	1,313.75	60.00
Chesapeake and Ohio Rail- way Company	Common stock, \$25 par	10	526.58	43.50
Total			\$5,482.48	\$233.21

REPORT OF THE MEMBERSHIP
COMMITTEE

New memberships in the Society during 1947-48 totalled 511. Of these new memberships, 262 are attributable directly to the efforts of the membership committee. The indirect influence of the committee is difficult to appraise, but it is real because members of the committee often encourage other members of the Society to undertake recruitment. Whatever may have been its total influence the membership committee can claim to have established what is probably the best recruiting record of the Society. In 1946-47 a total of 311 members were added of which 208 were due to the direct efforts of membership committee. The year of 1947-48 has witnessed therefore a substantially greater improvement.

A chairman who can report such achievement wishes there might be a way to give the entire Society a full picture of the work of each membership representative. Such work is difficult and the reward is nothing more than the pride which the representative may feel in developing the growth and strength of his major professional society. In this report I shall not try to single out outstanding achievement. I have made such a report to the members of the com-

mittee. Let it suffice to say that The American Sociological Society is greatly indebted to those members of the committee who have in the past two years helped to make the Society 50 per cent larger and stronger than it was before.

The chairman would like to record a few suggestions which his successor may find useful.

(1) The small budget which is now available to the chairman should be continued. In the hands of an active chairman the secretarial assistance which it provides more than pays for itself in memberships secured.

(2) The chairman should have a voice in nominating and approving the members of his committee. At present, such members have been appointed largely at the discretion of the President and Secretary. The chairman must then deal with many membership representatives, thousands of miles away, who may be strangers to him. If the chairman could have a strong voice in naming the members of his committee, there is reason to believe greater results could be attained.

(3) All membership materials (cards, literature, etc.) should be issued from the office of the chairman in order to expedite the work of the committee. All checks and application cards

should be sent directly to the Secretary-Treasurer (the current practice).

(4) A relatively large quantity of application material should be made freely available to membership representatives. In addition, such material should be actually placed in the hands of each member of the Society so that potential recruitment would be increased. The expense would be small in comparison with accruing membership receipts.

If one of the channels of strength of the Society is found through growth, then the above suggestions are strongly recommended. After three years of recruiting experience, one year as a membership representative, and two years as chairman of the committee, I believe that the four proposals are minimum essentials for more efficient operation.

Respectfully submitted,
DELBERT C. MILLER, *Chairman*

REPORT OF THE REPRESENTATIVE TO THE SOCIAL SCIENCE RESEARCH COUNCIL

During 1947 the Social Science Research Council has been very active in a number of matters which should interest members of this Society. As an introduction to the present report, I should like to quote a few paragraphs from the 1946-47 report to the Council by its Executive Director, Dr. Donald Young. He writes:

"The work of the Social Science Research Council during 1946-47, as from its beginning in 1923, was dominated by one central objective, the advancement of knowledge of human relations through research.

"Progress towards this objective requires planned effort. This has been more obviously true in the years following World War II than in earlier decades. Accumulated knowledge and recently developed research tools in the social sciences have made it evident that reliance on unplanned co-ordination of research by scattered individuals in the various social disciplines cannot be expected to lead satisfactory results. Research techniques have become increasingly specialized and difficult both to learn and to apply; critical research projects have become more expensive in dollars and manpower; demands by the government, by business, and by the general public for the products of the social sciences have outrun their existing capacities. Universities, research institutes, and government agencies offer potential resources for the operation of research projects far beyond their present

level of activity. Over-all research planning, however, requires at least one agency of national scope devoted to the interests of all the social sciences regardless of disciplinary or institutional barriers. The greatest opportunity before the Council consequently is in its research planning function.

"Research planning, as viewed by the Council, is a much more comprehensive task than the designing of specific projects for execution. Indeed, planning in the narrow sense, sometimes called 'blueprinting,' in inaccurate imitation of the natural sciences, is impractical without preliminary development of materials, techniques, and personnel with ability and skill in research. And so-called research 'blueprints' are of little use unless adequate facilities are available for putting them into operation. Research planning, then, means to the Council the entire process of developing research personnel, improving research techniques, mapping research opportunities and needs, designing research projects, assuring the existence of proper research materials, encouraging the investment of funds in research, and bettering the total circumstances under which research is conducted.

"The Council conducts its activities by means of four types of operating devices with distinctive responsibilities and ways of working which nevertheless overlap in function and are crucially dependent on each other: (1) a Board of Directors, (2) a Committee on Problems and Policy, (3) numerous committees with definite, limited mandates, and (4) a paid staff."

Besides the three members of the Board of Directors from the American Sociological Society (E. W. Burgess, L. S. Cottrell, Jr., and Kimball Young), the Problems and Policy Committee has three members from sociology, E. W. Burgess, who is chairman, and Donald Young, and P. M. Hauser.

Since many social scientists are not fully aware of the aims and operations of the Council, it has established a publication under the title, *Social Science Research Council Items*, more familiarly known as *ITEMS*. This publication appears quarterly and is available to all interested persons upon application to the Social Science Research Council, 230 Park Avenue, New York 17, New York.

The long-established fellowship and grants-in-aid programs continue. While the "Demobilizations Awards," designed to aid social scientists whose careers were interrupted by the war, has now been completed, the customary fellowship program, both for predoctoral and postdoctoral

training, goes forward. The Grants-in-Aid Committee has been active and the Directors continue to view this program as one of the important functions of the Council. The purpose of these grants is to aid individuals to initiate or complete particular research projects. As a rule the grants are rather modest, ranging from a few hundred dollars to a maximum of about \$1,000. It is interesting to note that, on the whole, as measured by applications, sociology has been rather under-represented in comparison with other social sciences.

There are a number of committees of the Council whose work is particularly pertinent to sociology. The one on the Measurement of Opinion, Attitudes, and Consumer Wants, joint with the National Research Council, and under the chairmanship of S. A. Stouffer, has been very active. During the past year it has carried further its pilot studies on problems of sampling under the guidance of F. F. Stephan. Also under the aegis of this Committee, the National Opinion Research Corporation, now located at the University of Chicago, is conducting a study on interviewing under the directions of Clyde W. Hart. A parallel project on the use of the panel system is under way under the guidance of P. F. Lazarsfeld at Columbia University.

The report on the contribution of the work of the Research Branch of the Morale Services Division of Army Service Forces, under the chairmanship of F. H. Osborn, is now approaching completion. The work itself is under the immediate supervision of S. A. Stouffer. Other sociologists who have been participating in this project directly are L. S. Cottrell, Jr. and Leland DeVinney.

The Committee on World Area Research has carried forward its program with considerable vigor under the leadership of Professor R. F. Hall, Department of Geography, University of Michigan. On November 28, 1947, this Committee held a large conference at Columbia University, with over 100 area specialists in attendance. Among these were a number of sociologists. Our members will also be interested to know that there has been established a series of Area Research Training Fellowships and Travel Grants to facilitate training and research in this whole field. The Council has already circularized social science departments and personnel over the country with announcements of these fellowships and grants.

The Committee on Government and Research in the Social Sciences has also given increasing attention to the nature and scope of social

science research now going on under governmental support. Also in this connection, the Committee has obtained the services of Professor Talcott Parsons of Harvard to write a report on the role of social science research on Government. A closely related interest of this Committee concerns the future role of social science in any national science foundation which may be set up in the future under federal legislation. In this connection, the Social Science Research Council is represented by Donald Young and Robert Yerkes on the Inter-Society Committee for a National Science Foundation.

The Committee on Adjustments for Old Age, under the chairmanship of E. W. Burgess, reports that another manuscript on this problem is now being read by this Committee with a view to later publication. The same is true of the Committee of Population which now has in hand a committee report by John Durand on "The Labor Force in the United States." Another important study now under way is one by Elbridge Sibley on problems of the "Recruitment and the Training of Social Scientists in this Country." A preliminary report of this appeared in *Social Science Research Council Items*, No. 4, December 1947.

Among important publications during the past year of interest to sociology are Louis J. Ducoff and Margaret Jarman Hagood, *Labor Force Definition and Measurement*, Bulletin 56, 1947, and Robin M. Williams, Jr., *The Reduction of Intergroup Tension: A Survey of Research on Problems of Ethnic, Racial, and Religious Group Relations*, Bulletin 57, 1947.

KIMBALL YOUNG

REPORT OF THE COMMITTEE ON THE TEACHING OF SOCIOLOGY IN THE PUBLIC SCHOOLS

The great need is for sociologists and other social scientists to work with public school teachers to build an integrated social studies curriculum from first grade through the junior college.

The extended report reviewed in detail current practices in elementary, junior and senior high schools. Though leading elementary schools offer comprehensive integrated curricula showing clear influence of the work of Wissler and the Lynds, and the use of the community approach, the most common curriculum is "atomistic," including a rather miscellaneous collection of materials dealing with community life, citizenship, heroes, geography, and history. The typical high school curriculum includes United

States history (repeated twice), civics, World history, and Problems of Democracy. There are few major variations in this program. Obviously, this is not an approach that promises to give youth an integrated understanding of contemporary civilizations as a whole. The high school curriculum needs the influence of the sociologists who see society as a totality of interdependent institutions.

Sociological Society should direct some of its efforts as follows: (1) keep in touch with developments in the social studies, (2) work on curriculum committees with teachers in local communities, (3) meet jointly (or attend) with the National Council for the Social Studies and/or cooperate with its endeavors, (4) attend local social studies teachers organizations and participate in the programs, (5) write texts more

TABLE I. COMPARISON OF A "TYPICAL" SOCIAL STUDIES CURRICULUM WITH A PROPOSED SOCIAL STUDIES CURRICULUM SHOWING THE SOCIOLOGICAL INFLUENCE

Typical Social Studies Curriculum		Proposed Social Studies Curriculum	
Grade		Grade	
I	Community Life, ¹	I	Neighborhood Community ²
II	Citizenship, American	II	Larger Community
III	Heroes and Holidays	III	The City (Town)
IV	Citizenship, American	IV	Physical Environments of the World
V	Heroes and Holidays	V	United States and Its Possessions
VI	Current Events, World	VI	World Wide Communities
	Geography, American Geography, American History		
VII	United States ³	VII	The Local Community
VIII	History	VIII	(Higher Level Analysis)
IX	Civics	IX	American Life and Institutions (Higher Analysis)
X	World History	X	History of the Americas
XI	United States History (Second Time)	XI	Contemporary World Civilizations (Higher Analysis)
XII	Problems of Democracy	XII	Family and Social Relationships

¹ First Six Grades from N. E. A., Department of Superintendents, "The Social Studies Curriculum," Fourteenth Yearbook, 1936.

² First Six Grades from Division of Elementary Education, Minneapolis Public Schools, "A Guide to Teaching the Social Studies in the Elementary School, 1943."

³ For High School Grades see Dorothy Meredith, "Secondary School Social Studies in 1945," *Social Education*, December, 1945.

Table I shows a comparison of a "typical" social studies curriculum with a proposed curriculum showing how sociology does and should influence it.

The committee believes that that proposed social studies curriculum has great promise and includes the study of interdependent social institutions, beginning with the simplest local institutions and working out to the world—this being done in two cycles, an elementary and secondary cycle. The only exception to this is the course on Family and Social Relationships designed to meet the immediate needs of high school graduates.

The committee believes that the American

sued to high school heads, (6) participate with other social scientists and social studies teachers in writing unified social science texts, (7) work for the inclusion of sociology in all teacher training curricula.

LESLIE D. ZELENY, *Chairman*
RUTH E. ARRINGTON
CLARK W. CELL
T. EARL SULLENGER
OSCAR WESLEY

PRELIMINARY REPORT OF COMMITTEE ON MONOGRAPHS

The Committee submits this preliminary report with recommendation for the following

action. First, it is recommended that the general sense of this report be accepted and that a similar committee be continued with instructions to make a complete and final report with definite specifications for the 1948 meeting of the Society. Second, it is suggested that the new committees working on the series be instructed to make a preliminary report for the special consideration of the Executive Committee at some interim meeting at the beginning of the spring semester in 1948 and that a mail ballot be authorized before making the final report.

It is recommended that the American Sociological Society, as one of its major undertakings, inaugurate a monograph series provided the following conditions are met. By the same token, it is urged that the Society should not undertake such a venture unless these conditions can be met.

1. There should be relative unanimity on the part of the Society as to the importance and distribution of such a series.

2. There should be commitments on the part of members of the Society to support the series on the basis of at least a five-year guarantee.

3. This support should include prospective offerings by members of the Society of enough voluntary manuscripts duly qualified to insure a distinguished series at the end of the period.

4. This support should include an actual voluntary fee or subscriptions for all monographs by members and friends of the Society insuring the minimum guarantee of publishing costs. The point of emphasis here is that the Society must believe in its monographs and must insure both continuity and quality.

5. It is suggested that a mail ballot be authorized to vote on the question of the amount of added fee or subscription to be specified.

6. Definite specifications should be complete for publishing arrangements, including sample contracts from publishers, guarantee of uniformity and size in format, quality of paper, length, etc.

7. The Society's organization should include a committee on manuscripts, a committee on finance of monograph series, and a director of publications.

8. The specifications for the monographs must provide not for just the publication of "papers" or general research but each monograph must comprehend a central significant theme, insuring an actual contribution in a designated area of research.

9. In general, however, the monographs should be so written, edited and published as

to carry not only prestige for sociology but a certain appeal among scholars.

10. The series should be so well done and so esteemed as to make publication in the series something to be sought after and to carry a certain reward of distinction and/or royalties.

HOWARD W. ODUM, *Chairman*

F. STUART CHAPIN

MIRRA KOMAROVSKY

KIMBALL YOUNG

REPORT OF THE COMMITTEE ON RELATIONS WITH SOCIOLOGISTS IN OTHER COUNTRIES

The Committee had two main tasks: (1) to consider in what manner and to what extent the Society could cooperate with UNESCO or be of service to it, and (2) to canvass the question of the relations between the Society and other international organizations in its field, and at the same time to consider the situation with respect to the present international organization of sociologists. The Committee wishes to report certain recommendations under these two heads, but before doing so, it desires to offer a general recommendation. It found that the fulfillment of the functions for which it was appointed would require the continuous interest and attention of the Society, and, therefore, it proposes as follows:

- (1) That a continuing committee of three members of the American Sociological Society be set up for the purpose of establishing and developing the international interests of sociologists with the understanding that they will consult with such other members of the Society as may, from time to time, seem desirable.

That this committee report from time to time to the Executive Committee of the Society as well as to the annual meetings and that it formulate and carry out a program as may be authorized by the Executive Committee and the Society.

- (2) On the matter of the relationship between the Society and UNESCO, the Committee offers the following recommendations:

That the proposed continuing committee keep in regular touch with UNESCO and, also, with special agencies of the United Nations, particularly in the following ways:

- (a) By defining areas and problems of international relationship with which sociologists are particularly competent to deal, and by offering its cooperation to UNESCO and these other agencies when and in so far as the matters thus defined are under their consideration.

(b) By taking cognizance of projects actually being considered by UNESCO and exploring how far available sociological research or experience on these subjects can be made available to UNESCO through the Society.

(c) By considering how far sociological researches made in this country might with advantage be simultaneously initiated in other countries, so that these issues may be seen in their international significance.

(d) By offering to review through the resources of the Society such problems as come within the special competence of members of the Society.

(3) On the question of the formation or re-formation of an international society of sociologists, the Committee offers the following recommendations:

(a) That the Committee consider the steps that would have to be taken to develop an international organization of sociologists.

(b) That in any procedures for the achievement of this purpose due consideration be given to existing organizations in this field with a view to their incorporation within the program.

(c) That in preparing the laws and by-laws of this inclusive organization the experience of existing international associations be drawn upon.

(d) That every effort be made to turn the new organization into a vital and generally operative concern, and that to this end membership should not be regarded as an honorific tribute to elder statesmen. Furthermore, the program of the organization should include international conventions, with working groups in the various countries, devoted to the advancement of sociological knowledge and to sociological problems of international import.

R. M. MACIVER, *Chairman*

E. FRANKLIN FRAZIER

PHILIP M. HAUSER

CURRENT ITEMS



COMMUNICATION AND OPINION REJOINDER TO DR. HORNSETH'S NOTE ON "THE MEASUREMENT OF ECOLOGICAL SEGREGATION"

The major point set forth in Mr. Richard Hornseth's critique (*American Sociological Review*, October, 1947, pp. 603-4) of our paper on "The Measurement of Ecological Segregation" (*Ibid.*, June, 1947, pp. 293-303) is that three of the four measures are "redundant." This argument is based on a mathematical demonstration that all four indices represent differences between "observed" and "expected" proportions of Negroes (or any other population group or class) in census tracts: two are based on 2×2 tables and two on $2 \times k$ tables. Mr. Hornseth concludes, therefore, that the indices are "for practical purposes identical measures" and that the "subtle distinctions between them have no correlates in the sociological problem of segregation."

We were well aware of the similarity of the four different measures which we presented. In fact, considerable space was devoted to this question (see especially pp. 297-302). If it could be demonstrated that one index were a simple function of another so the value of one could easily be determined by the value of the other, then one of the measures could perforce be regarded as redundant. Mr. Hornseth does not show that any one of the indices is a simple function of any other. I_2 and I_3 are not simple functions of the 2×2 table indices nor are they simple functions of each other. And since I_1 and I_4 define "segregated area" in different ways, we see no necessary redundancy. Whether or not the various indices can be used interchangeably will depend upon the problem under investigation, as well as upon additional empirical research. Furthermore, the comparative advantage of any one index over the others will depend largely upon the particular problem at hand, as we previously indicated.

It is difficult, if not impossible, at this time to make broad generalizations concerning the specific value or application of the respective

indices. Each index fulfills the criteria we set up in the original paper. There we were primarily concerned with constructing an objective statistical index or measure, logically corresponding to the concept of ecological segregation. However, we found that a number of different measures can be constructed, all corresponding logically to the concept of ecological segregation, but not correlating perfectly with each other when applied to the same data. In order to determine which index should be used under given conditions two additional criteria are suggested: First, *the criterion of prediction*: Given a defined variable to be predicted within a defined universe, and given certain statistical principles and methods of prediction to be used, that "Index of Segregation" which exhibits the highest accuracy of prediction under the defined conditions should be accepted for use. Second, *the criterion of reproducibility*: Given certain statistical data that have been observed and tabulated in an acceptable manner and given the purpose of summarizing these data by one or a few numbers with a certain allowable amount of error, that "Index of Segregation" which enables the researcher to "reproduce" the original data most accurately should be accepted for this use. (For a discussion relevant to these two criteria, see Guttman, Louis, "An Outline of the Statistical Theory of Prediction," in *Social Science Research Council*, Bulletin No. 48, pp. 253-349, and "A Basis for Scaling Qualitative Data," *American Sociological Review*, April, 1947, pp. 139-150.)

If the criterion of prediction is accepted, the correlation data published in our previous paper are sufficient to show that the four indices are not necessarily identical or interchangeable. Further research is necessary, however, in order to determine the relative accuracy of prediction of these or other indices under defined conditions.

If the criterion of reproducibility is accepted, the intercorrelations among the four indices show that they do not reproduce the same data with the same accuracy. Again, it requires further research to demonstrate which of these or other

indices most accurately reproduces the data to be summarized.

It also should be noted that Mr. Hornseth's claim that I_4 does not vary between 0 and 1 is in error. By definition I_4 equals 1 when no Negroes live in tracts in which non-Negroes live. In the example he gives, both Negroes and non-Negroes live in tract 2, and we would not expect the score to be 1. What we have in I_4 is the difference between the proportion of the total Negro and the total non-Negro population residing in the "segregated area," and it can be shown mathematically to have 0 and 1 as its limits under the conditions we defined.

In addition to the foregoing arguments, Mr. Hornseth unfortunately indulges in a few gratuitous and irrelevant remarks. What the "sociological correlates" of the "subtle distinctions" among the indices might be, or whether they are "for practical purposes identical," is a matter to be solved by empirical research and not, in our opinion, by authoritarian decree.

JULIUS A. JAHN
CALVIN F. SCHMID
CLARENCE C. SCHRAIG

University of Washington

NOTE ON "QUESTIONNAIRE VS. INTERVIEW METHODS IN THE STUDY OF HUMAN LOVE RELATIONSHIPS"
BY ALBERT ELLIS

In the October, 1947, issue of the *American Sociological Review*, there appears a study by Albert Ellis purporting to evaluate the relative revealingness of questionnaire and interview techniques with respect to love life data. ("Questionnaire versus Interview Methods in the Study of Human Love Relationships.") The conclusion is reached "that in investigations of love and marital relationships with college subjects, the questionnaire method of getting data is at least as satisfactory as the interview method; and that as questions become more ego-involving, the questionnaire technique may produce more self-revelatory data than the interview method" (p. 552). Several pages are devoted to corroborative evidence. Such a conclusion is, of course, very welcome, and one which most research students would be glad to accept unequivocally, inasmuch as questionnaires are less time-consuming than interviewing. Unfortunately, however, the project described in the *Review* does not render acceptance of such a conclusion legitimate.

I do not wish to cavil or to appear unduly critical of any sincere research project, especial-

ly one which shows so much evidence of painstaking and laborious work. But inasmuch as erroneous designs for research projects occur so frequently in sociology it seems worth while at this time to protest against them in order to put us on our guard against such fallacies in the future. Otherwise a tremendous amount of labor is expended uselessly.

In the study under discussion, for example, it is assumed because questionnaires brought forth more revealing results than interviews from a constant sample after the lapse of a year's time, that this fact *ipso facto* proved the questionnaire method more effective in bringing out data with respect to love life. The results of what psychologists call practice effects were not, however, adequately considered in the planning of the project. This fact vitiates the whole study.

A properly designed project to bring out the relative effectiveness of questionnaire and interview in bringing out data on the love life of individuals would proceed as follows: (1) the original sample would be sub-divided into four matched or equated samples; (2) two of these sub-samples would be interviewed; (3) two of the sub-samples would be approached through questionnaires; (4) after a year, one of the samples originally interviewed would be approached through questionnaires; (5) the other sample originally interviewed would be reinterviewed; (6) one of the samples originally approached through questionnaires would be interviewed a year later; (7) the second sample originally approached through questionnaires would be re-questionnaired. Thus:

	A	B	C	D
First Approach	Q	Q	I	I
Second Approach	I	Q	Q	I

Now if the hypothesis is that, with respect to measures of revealingness, $Q > I$ we should expect this relationship to obtain in sample A as well as in sample C (which represents the situation reported in the article here under discussion), and we should expect sample B to show substantially greater degrees of revealingness than its matched or equivalent sample D, in both the first and the second approach. If such differences were found to obtain in statistically significant measure, we would feel a good deal less reluctance in accepting them as valid. If one protests that the work here suggested is excessive, one can only reply that there are no shortcuts in science. There is no royal road to scientific truth any more than to geometry. If one wishes to know definitively which procedure

calls forth more revealing data, questionnaires or interviews, one has to be willing to go through the whole process of scientific testing. Neither industry nor factual prodigality is a substitute.

The publication of projects containing such fundamental methodological errors raises a question with respect to editorial policy which, it seems to me, ought to be openly debated by interested sociologists. Inasmuch, for example, as the cover of the *Review* proclaims it the official organ of the American Sociological Society, is it not incumbent upon the editors and editorial readers to screen out work that does not reach certain minimum standards of scientific competence? Or should they not at least suggest to authors how to raise their work to such standards before publication? With the pressure on publication space in professional journals, is it good policy to use space for articles that do not measure up? The time is now past, or ought to be, when just any research project was acceptable, because there were so few and so little choice. We ought now to feel that any project reported in a professional journal was worthy of serious scientific consideration. With the tremendous amount of reading required nowadays to keep up with one's field, it seems not an excessive request to make of editors that they help reduce the job by careful editorial screening. One more example of the need for such editorial services: I had not myself noticed the redundancy in the four indices of ecological segregation reported by Schmid *et al.* in the June *Review*. I was grateful to have it pointed out by Hornseth in the October issue. But, could it not be argued, should this redundancy not have been pointed out before publication, rather than after, so that whatever modifications in presentation were called for could have been made in the original article?

It may be that the present policy of the *Review* precludes such rigorous editorial screening. Could not this proposal, nevertheless, receive consideration by readers of the *Review*?

JESSIE BERNARD

The Pennsylvania State College

DISCUSSION OF MRS. BERNARD'S COMMENTS

With Mrs. Bernard's general thesis, I find myself in close agreement. Her main points seem to be these: (1) that great care should be taken in the proper experimental design of research studies; and (2) that when reports of inadequately planned or executed projects are made, these should be carefully screened by the

editorial board of the *Review*, and either rejected or resubmitted to the author for drastic revision. I am so heartily in agreement with these points that I should like to see them applied to *all* scientific publications (if this is indeed not already common editorial procedure) and not merely, as Mrs. Bernard suggests, to journals like the *Review*, which are the official organs of some particular scientific society.

While agreeing with Mrs. Bernard's main thesis, I am afraid that her choice of my study as an illustrative example was unfortunate. In the first place, she attacks my experimental procedure because "the results of what psychologists call practice effects were not . . . adequately considered in the planning of the project." But *practice effects*, as Mrs. Bernard fails to note, have never been shown to be an important issue in the *particular kind* of verbal material employed in my study. It is perfectly true that if a mental test or an achievement test is given to a group of subjects on one occasion, and then a similar test is administered to the same subjects after a certain interval, practice effects may be expected. But it is difficult to see how such practice effects significantly influence questionnaire and interview material like that employed in my study. Does Mrs. Bernard seriously believe that asking a subject, on one occasion, how much she loved her mother during her childhood, or how excited she was when she fell in love with a certain male, will cause her to give more practiced, and presumably better, answers the next time she is asked the same question? If that were so, the results of my study, which showed that the subjects actually tended to give *less* favorable responses on the second (questionnaire) administration, would be even more significant than they seem to be.

What Mrs. Bernard does not appear to realize is that while no competent investigator would determine the reliability of a mental test by retesting his criterion group with the same instrument, this is a well-accepted and scientifically accredited practice in determining the reliability of questionnaire material. Apparently, the best available psychological evidence discounts the possibility of significant practice effects in the administration of *this particular kind* of material, even though their influence on certain *other* kinds of material are indisputable.

Moreover, as Mrs. Bernard apparently failed to observe, the experimental plan of my study was such that the second administration of the questions to identical subjects took place *on*

entire year after the first administration. The probability of practice effects, even had they been expectable over a short test-retest interval, still exerting a significant influence after a whole year's lapse, seems to be quite negligible.

Secondly, it should be pointed out that an experimental procedure similar to the plan which Mrs. Bernard suggests for my study was considered when the experiment was originally planned, but had to be rejected, aside from the fact of its being unnecessary, because the very nature of the experimental conditions made its execution impossible. For the study, as Mrs. Bernard seems to forget, was a comparison of the use of an *anonymous* questionnaire and a non-anonymous interview. Now, while it is quite possible to obtain the names and addresses of interviewees, later to mail these interviewees an anonymous questionnaire, and then to match the returned questionnaires with the original interview data (by checking such items as place and date of birth, amount of schooling, parents' birthplaces, etc.), it is not very possible to reverse this procedure. If the anonymous questionnaires are received first, especially by the mail method which was used in my study, it is almost impossible to identify the subjects, and to trace and interview them a year later. Consequently, the experimental design suggested by Mrs. Bernard, which (even though it did not seem to be required by the nature of the experimental material) would certainly have done no harm, and which possibly might have led to the gathering of additional interesting data, had to be rejected because of its impracticality in this specific experiment.

In conclusion: while the experimental design suggested by Mrs. Bernard is theoretically an excellent one, it did not seem to be warranted by the particular nature of the materials used in my experiment; and the nature of the experimental hypothesis made it, anyway, virtually impossible to execute such a design even had it been desirable. In no respect, contrary to Mrs. Bernard's allegations, do the conclusions of the study seem to be vitiated. This, however, does not negate the fact that Mrs. Bernard's general points concerning the execution and the publication of research studies probably have a considerable degree of validity.

ALBERT ELLIS

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EDITOR'S NOTE

The following has been the practice for screening manuscripts during my editorship. A

manuscript is first assigned to that one of the four members of the editorial staff most competent to deal with it. He returns it to me with a written evaluation. I then read the manuscript. If he thinks it definitely worth publishing or definitely not worth publishing, and I agree with him, that settles the matter. If we disagree, or if we both think the manuscript is marginal, we send it to an Assistant Editor, whose judgment is then final. When the shortcomings of a manuscript are minor, we make suggestions for revision. Several manuscripts have been revised in the light of our criticism and have subsequently been published.

ROBERT C. ANGELL

SOME COMMENTS ON SOCIAL TRENDS

In the June, 1946 issue of the *American Sociological Review* an article on social trends prepared by Hornell Hart of Duke University was published.¹ Though the charted data in each instance involved decelerating plots, equations were fitted which produced curves indicating acceleration to be the predictive possibility.

Justification for this procedure was the assumption that the evidence for deceleration did not reflect the historical picture. No adequate evidence, however, was produced to support the assumption. The Hornell Hart assumption that existent knowledge is sufficient to promote further permutations and combinations in inventive activity can only be used to evaluate immediate futures. Historical futures involve permutations and combinations of factors only indirectly related to inventive activity. The very presence within society of a vast mass of undigested and unorganized knowledge, for instance, is evidence which suggests that inventive and adaptive efforts will become increasingly absorbed in the problem of clarifying and applying available insights rather than in creating outstanding new developments.

Generally, one can ignore claims that society is disintegrating because of its failure to adequately adapt. Such reactions are the reflection of man's constant difficulty in adjusting to changing conditions. However, the evidence that such adjustive problems are forcing attention to the problems of absorbing past developments cannot be ignored. Note the present international adaptive difficulties. If past records are any index, such historical evidence would seem

¹ Hornell Hart, "Technological Acceleration and the Atomic Bomb," *American Sociological Review*, XI (June, 1946), 277-293.

to be a sound basis for assuming historical deceleration. Such an assumption is reinforced by noting the tendency for deceleration to succeed stages of acceleration.

Some of Hornell Hart's commentary suggests that that was actually his assumption. However, his procedure was postulated on the basis that rather than adjustive tendencies setting in that man is heading toward catastrophe by accelerating his inventive achievements rather than incorporating them.

In the following examination of five of the phenomena utilized by Hornell Hart, the discussion is directed to two points. First, to indications warranting an assumption of deceleration, and second, to the evidence indicating an over-stress of adjustment problems involved in some of the data. For present purposes, the indications that they are not commensurate data are ignored.

Life Expectancy. It is true enough that the average life expectancy of the white American has been pushed beyond the 60's, but that is not true of all population elements within the United States. And it certainly has no bearing on the remainder of the world. Since the United States represents only seven per cent of the population of the world, life expectancy of its population is not a conclusive basis for any discussion of historical trends. As a factor in maintaining the high level of technical efficiency in the United States, it has more pertinence in a discussion of world relationships.

Furthermore, since life expectancy is a consequence of the conditions of man's life, it does not seem to be a predictive index as much as it is an index whereby past conditions can be evaluated in terms of human physical welfare. Even though further improvements are possible, problems of world economy indicate that it will require tremendous outlays of energy and skill to bring about any rapid change. Such effects would apply primarily to populations outside the United States. For the United States, the pressure on the upper limits of life expectancy are indicated by the increasing expenditures on research for cures to malaria, cancer and other diseases, and by the fact that any basic increase in the limits of life expectancy depends upon the discovery of knowledge which will change the basic life potential of men.

Divorce. Certain factors in the American scene seem to indicate that the picture is not as serious as assumed by Hornell Hart, for American attitudes seem to have completed a

cycle. Beginning first with a breakdown of assurance in the validity of the marriage system prior to and during World War I, it went into a cycle of discussion of free love and other concepts intended to adjust the relationship. In turn, doubt of the adequacy of such formulas developed, and experiments in notions of fifty-fifty adjustments were formulated. Now the search has swung toward efforts to resolve adjustments on the basis of the personalities involved rather than according to any overall formula.

How soon such developments will formulate clearly enough to establish permanence as a characteristic of the legal union known as marriage is another question. If the adjustment is not at a rate of acceleration greater than the rate of disruptive factors, the Hornell Hart extrapolation will prove correct. However, since the adjustive patterns are evident, an expectation that the rate of divorce should decrease is not unwarranted and seems to be supported by isolated evidence.

Speed. At present, commercially feasible speeds for planes probably does not average 300 miles an hour. Considering everything, it is, indeed, doubtful if the average speed of planes exceeds 100 miles an hour. To raise such an average markedly actually does not at this time depend on increasing the speed of planes, but in developing more effective techniques of co-ordinating transport. Such co-ordination would have more effect in raising average speeds than would the development of the type of air field needed to launch high speed planes.

Thus, even though supersonic speeds were immediately feasible, it is doubtful if they can materially effect the commercial speeds of society. In fact, when one considers that the overall speed with which the United States carries out its transport probably averages in all branches far below ten miles an hour, one must conclude that the extrapolation toward sixteen hundred miles an hour does not reflect a basic social validity but an experimental extreme.

Projectiles. Basically, there is nothing new about the movements of peoples and goods over thousands of miles. The use of planes has not changed the pattern, but only speeded up certain areas of this intercommunication. For obvious reasons, it has done so primarily in high priority areas. A further restriction on the development of long-range, non-stop movements of goods and people is the lack of need for such costly transport.

Naturally, by eliminating the barrier of time which isolated men and prevented the necessity of mutual adjustments, antagonisms and the lack of adjustments will become more important. However, such sources of tensions need not develop into basic tensions. Actually, the world-wide impact of western civilization is more strongly felt as the result of slow-moving goods than as a result of goods transported at high speeds over long distances.

Explosives. Since fission of the atom is a factor which cannot be evaluated in detail, the confusion concerning its significance is directed more toward its unknown potentialities than toward its immediate significance. Despite its potentialities, it is as yet little more than a destructive force. How rapidly it will become a significant positive factor in the conduct of human affairs is one of the unknowns.

In terms of its potential as a weapon of war, it involves the same limitations as any total weapon. If used in limited warfare involving large scale movement of ground troops, it not only involves the risk of retaliation but involves the same handicap as did gas. Thus, the atomic bomb practically negates itself except as a weapon for last-ditch defense or for world-conquest. The real significance of atomic fission seems, indeed, to lie in its peaceful applications. Certainly, it is not particularly effective as a factor in present political maneuvering.

Conclusions. Deceleration seems to be present as a factor in virtually every phase of social behavior. A complete examination of the situation would require an examination of the pattern of potentiality, but the co-ordinated view from the various fields of activity indicate that in a broadening sweep further significant development depends on completing and resolving established developments.

Despite some impressions, technical efficiency is no more significant in this situation than organizational efficiency. A small nation, Germany, because of its efficiency of organization and calculation of international potentials came near achieving a world-wide reversal of power relationships. The United States because of organizational skill succeeded in feeding and clothing itself, equipping its own army, and in addition supplied other nations around the world.

The true situation must be assumed to be that technical proficiency and organizational skill are both far superior to the demands of the areas in terms of which they function, but that neither are far enough advanced to meet the

demand for an integrated world. There are too many short-comings in our present capacity for record keeping and analysis, too much difficulty in our present communicative systems, and as a direct result too much man-power is needed to co-ordinate and handle records and carry out administrative duties to permit any international system.

At the same time, because the level of skills are so high, existent patterns of adjustment are instable. This instability is functioning at every level of contact. One may note the present drive of India and every other area to develop as industrial nations. Implicit in the situation as a totality are complex disrupting adjustive problems with world-wide significance. These difficulties did not arise out of present developments, but past developments which made possible nuclear fission and it may develop that only maximum development of the implication of nuclear fission will be sufficient to resolve the adjustive problems of the world.

In terms of this evaluation, Hornell Hart's charts are contrary to implicit and explicit trends. Rather than increased acceleration representing the social problem, the factors suggesting deceleration would seem to contain the problem. And this adjustive problem is not merely a matter of social adjustment as so many assume, but as the problem of food, transportation, minerals, etc. indicate, it is also a problem of technical adjustment.

Actually, even if one accepted the notion of acceleration in the Hornell Hart charts, one would have to point out that they involve one important error. The zero assumed is the zero of pre-history. It implies that prior to the recent upswing in history men were capable of meeting their problems but have not advanced since then. The assumption is doubtful, even as the failure to include the long pre-history of every development as an element in evaluating the rate of development, is a doubtful postulate.

Unfortunately, no one is able to state clearly as yet the actual level of man's social zero, and therefore no one is able to state to what degree the factors of acceleration or deceleration are operating. The world-wide spread of scientific and technical knowledge including that derived from the social sciences indicates that the zero is higher than some are inclined to assume. And yet, it is probably not as high as would be desirable.

Without an answer to this problem, it is not possible to make any reliable estimates as to the

time required to solve present adjustive problems of the world. There seems, therefore, to be little profit in attempting to deal with it. Only by resolving immediate adjustive problems can the historical factors leading to centuries long deceleration be negated. It would seem more profitable to develop postulates directed to insight into immediate problems of adjustment upon which the issue of acceleration and deceleration both depend.

ARTHUR G. LINDSAY

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NEWS AND ANNOUNCEMENTS

Centre d'Études Sociologiques, Paris, is organizing a Sociological Conference, June 3-5, 1948. The topic to be discussed is "*Industrialisation et Technocratie*."

Japanese Sociological Society. The Society's twentieth convention was held at Tokyo Imperial University in October, 1947. More than 150 persons attended the sessions. Three guest lectures were given by John Pelzel on "Applied Anthropology in the United States," Herbert Passin on "Social Psychological Dimensions of Public Opinion," and Wu Wen-Tsae of the Chinese Mission on "The Development of Sociology in Wartime China."

American Catholic Sociological Society. Dr. Franz H. Mueller of the College of St. Thomas, St. Paul, Minnesota, was elected President of the Society at its meeting in St. Louis, January 30-February 1, 1948.

The Journal of Legal and Political Sociology has ceased publication with Volume IV. M. Georges Gurvitch, the Editor, has returned to France. He is now editing *Cahiers Internationaux de Sociologie*.

The Social Science Research Council announces Area Research Training Fellowships and Travel Grants for Research in World Areas for 1948. These are designed to facilitate research calculated to increase the understanding of cultures outside the United States. Although the fellowships and grants will normally be awarded for work abroad, a period of preparatory study in this country will be permissible for those who wish to improve their knowledge of the area in which research is planned. Three types of awards will be made: Predoctoral Area Research Training Fellowships, applicants for which must have fulfilled all Ph.D. requirements except the dissertation; Postdoctoral Area Research Training Fellowships, applicants for which, preferably less than 35 years of age, must be planning research careers in a foreign area; Area Research Travel Grants, to be given to mature area specialists. Stipends will be set by the committee on selection. The maximum amount awarded for a

travel grant will be \$2,500. Address inquiries to Office of the Fellowship Secretary, Social Science Research Council, 230 Park Ave., New York 17, N.Y.

George Davis Bivin Foundation. The purpose of this organization is to promote an interest in the mental hygiene of children. This is accomplished in two ways: by aiding with Fellowships worthy graduate students who are preparing to enter this field, and by helping colleges and other organizations promote meetings and institutes dealing with the subject. Direct any inquiries to Millard L. Jordan, Secretary, 1983 East 24th St., Cleveland, Ohio.

The Sociology Club of Pittsburgh, now entering its seventh year, has elected officers for the year 1948. These are: Joseph H. Bunzel (Washington and Jefferson College) President, Mabel A. Elliott (Pennsylvania College for Women) Vice-President, and Walter Sherman (Federation of Social Agencies) Secretary-Treasurer. The Executive Committee consists of past presidents, namely: Verne Wright (University of Pittsburgh), Maurice Moss (Urban League), Joseph Homer (Juvenile Court), and Maurice Schulte (Duquesne University). The club plans for the new year a series of discussions on Theory and Research in Science.

Columbia University. A new leadership training and intensive research program in intergroup and intercultural education to be supported for 5 years by a \$125,000 grant from the National Conference of Christians and Jews will be launched next fall by Teachers College. The program, involving a new inter-divisional major at Teachers College, is intended primarily for students beyond the Master's degree and will offer sizeable scholarships and fellowships, the application procedures to be announced later.

Central Michigan College of Education. Dr. Philip M. Smith has been appointed Professor of Sociology, and Mr. Harry R. Doby has been added to the staff as Assistant Professor. Dr. Margaret Koopman, who was on leave of absence last year while doing educational work in Germany, is again handling courses in educational sociology.

Harvard University. Applications will be received up to June 1, 1948 for the Jacob Wertheim Fellowships in the field of industrial relations. These Fellowships are awarded to applicants submitting a program of research which may normally be expected to result in publication. The awards are made for a year but may be renewed for a second year in exceptional circumstances. Residence is normally expected in Cambridge, Mass., although reasonable field work is appropriate, and an allowance for travel may be granted.

Any mature man or woman with expert knowledge or experience in the field of industrial relations

may apply. A university degree is not requisite. Each applicant should file five copies of the following statements with the Wertheim Committee (Professor John D. Black, Chairman, 205 Littauer Center, Harvard University, Cambridge, Mass.): (a) A statement of the project for research, including a detailed outline of the proposed investigation and the principal sources of information; (b) A full statement of the experience and education of the applicant; (c) A list of five references conversant with the applicant's capacity for mature work in industrial relations.

Roosevelt College, Chicago. St. Clair Drake will be on leave during the Spring semester, continuing research in England under his Rosenwald Fellowship.

Arthur Hillman, department chairman, was elected a trustee of Roosevelt College in the Fall. Five faculty members, chosen by their colleagues, serve with fifteen public members on the College Board. The latter include Edwin R. Embree, its chairman, and Lyle M. Spencer, President of Science Research Associates.

Several lecture series have been conducted on questions of sociological interest. Among the speakers in a series on Race Relations were Rayford

Logan, Howard University; Joseph D. Lohman, University of Chicago; and Arnold M. Rose, Washington University.

University of Chicago. A Workshop on Family Life Education is being held August 2 through September 3, 1948, for selected leaders currently active in school and community programs of education for family living. Evelyn Millis Duvall is Director, and shares seminar leadership with Ernest W. Burgess and Robert J. Havighurst. The over-view lecturers who have been invited to participate include Drs. Alfred Kinsey, William Menninger, Leland Stott, Ralph Tyler and Alvin Zander. Further information may be secured from the Workshop Secretary, University of Chicago, Chicago 37, Ill.

University of Michigan. The Research Center for Group Dynamics, organized at the Massachusetts Institute of Technology by the late Kurt Lewin, is moving to this University in the summer of 1948. Dr. Ronald Lippitt, one of the four members of the research team, has been appointed an Associate Professor of Sociology and will give graduate work in the field of the dynamics of small groups.

BOOK REVIEWS



Sexual Behavior in the Human Male. By ALFRED C. KINSEY, WARDELL B. POMEROY, and CLYDE E. MARTIN. Philadelphia and London: W. B. Saunders Company, 1948. xvi 804 pp. \$6.50.

Recently when it fell to this reviewer to summarize several statistical studies of sexual behavior, he, knowing of the imminence of the present work, felt like a boy shooting fire crackers on the eve of Bikini. By way of further analogy to the now well-known bomb, this book should have been called the "Sexual Behavior of the American Male," for that is what it is about.

The man back of this gigantic project, which is still only in its infancy (12,000 cases recorded out of 100,000 contemplated) is Alfred C. Kinsey, a taxonomic zoologist, of Indiana University. His earlier research was partly devoted to the measurement and classification of insects. Also quite in keeping with his passion for facts more than fame is the bringing of his two principal research associates into partnership as authors of the book itself.

The research is sponsored by the Committee for Research on Problems of Sex of the National Research Council, and has been financed in major part by the Rockefeller Foundation, with moral and material support also from Indiana University.

The study is a contribution to statistical procedure, and includes a chapter on "Interviewing" which is worth more than most of the treatises devoted primarily to this subject. The recording method is a real invention: super-abbreviated, top-secret symbols whose meaning depends on their position in a memorized frame. These authors are convinced that the destructive effect of recording during an interview upon rapport, is due entirely to the slowing down of face-to-face interaction by the usual lengthy methods of note-taking.

In the interviews the policy was to hold meanings uniform and permit words to vary according to the language and culture of the subjects. The point, not the form, of a question is standardized. In each interview practically complete coverage was obtained of a minimum of

about 300 points; the report is based upon 5,300 male subjects.

The primary purpose was not to get a properly weighted sample of the whole population, as in public opinion studies, but to get adequate samples of many categories of population, each homogeneous in as many ways as possible. By using census weights, however, the authors convert many of their findings into overall estimates for the total white male population over 15 years of age (universe = 44.7 million). The population is subdivided by sex, race-cultural group, marital status, age at adolescence, educational level, occupational class (10 categories here), occupational class of parent, rural-urban background, religion, degree of religious adherence, and geographic origin.

The main object of study is "sexual outlets," which are strictly defined as orgasm and ejaculation. All outlets are classified as masturbation, nocturnal emissions, heterosexual petting (to climax), heterosexual intercourse (pre-marital, marital, extra-marital), homo-sexual outlet, and animal contacts. The average American male, all ages considered, is estimated to have 2.34 sexual outlets per week. The peak frequency is for married males of 16-20, 4.8 times per week; from then on it declines steadily. For all males the frequency stands at a plateau of 3.4 until age 30, then declines in a straight line to 0.6 at age 70. Until 30 the decline of physiological urge seems to be just about balanced by the increased opportunities gained by entering into marriage. At ages 21-25, marriage apparently raises the average frequency of all heterosexual intercourse from 1.37 to 3.73 per week, and of total sexual outlets from 3.04 to 4.14. By age 74 about fifty per cent of men have become impotent. There is no sudden drop in middle age, no support for the theory of a "male climacteric." Early onset of adolescence correlates strongly with high frequency of outlets during the 35 or 40 most active years, but not significantly with the age of reaching impotence.

By the age of 25, about 83 per cent of all unmarried males (but only 64 per cent of those of college education) have had pre-marital in-

tercourse, 93 per cent of all males have masturbated, 35 per cent of all males have had homosexual experience. More indirectly and roughly the authors estimate that probably a half of all married males have had extra-marital intercourse (i.e., at some time while married).

Most striking are the class differences. Pre-marital intercourse is much below average at the higher educational and occupational levels; masturbation, nocturnal emissions and petting to climax are correspondingly higher. The attitudes toward these practices show corresponding class differences. Homosexual experience is greatest at the intermediate (high school) educational level. These differences are largely compensating, so that in total sexual outlet there are only moderate class differences without any linear correlation. The picture argues for physiological determination of quantity of outlet with cultural determination of the manner of outlet.

The authors seem at a loss to explain why extra-marital intercourse drops from an original 12 per cent to 6 per cent at age 50, of the total outlet, at the grade school level, while it is rising from 3 to 14 per cent of the total outlet during the same age span of the college level males. They have elsewhere hinted a partial explanation in the lesser sexual responsiveness, on the whole, of college level women. But they seem to overlook the obvious socioeconomic situation, namely that upper level males are surrounded by a surplus of unmarried, divorced, and widowed women, and also have more money and freedom to facilitate contacts with these women, which conditions become more favorable with increasing age. The lower level older male has fewer women available and less means to maintain his personal attractiveness.

While the study is concerned mainly with more concrete phenomena, there is considerable attention to the attitudes and ideologies of sex. Here the reviewer must summarize and interpret points somewhat scattered through the book. On the upper social level the value of "chastity" is now so defined that many can pet to the point of orgasm without feeling they have violated it. While it has lost much of its religious and authoritarian quality, it has been bolstered up by the values of good taste and good manners, reputation, etc. It takes time and money to achieve intercourse with companions in the upper level. Also, in that group masturbation is less seriously condemned. This permissiveness may have been increased by the sex enlightenment which has more greatly influenced the educated classes. At the lower level intercourse

outside of marriage may be just as much, or even more, recognized as "wrong" or "sinful," but it is worse to be "perverted" than to sin. There is a large body of opinion on the lower levels that masturbation, petting without completed intercourse, deep kissing, elaborate foreplay, and nudity are somehow obscene, disgusting, or perverted. Marital intercourse is had regularly in the nude with about 90 per cent of college level males, 66 per cent of the high school level, and only 43 per cent of the grade school level. The authors say this is not due to the inconveniences or lack of privacy of the lower class home, but to attitudes; they found some older men and women of this group who took pride in never having seen their spouses nude. However one does not have to be a complete Marxian to see possible economic origins of this differentiation of attitudes.

The authors estimate that for perhaps three-quarters of all males, orgasm is reached within two minutes; prolongation and foreplay are chiefly upper class phenomena. Nevertheless the rank and file of women appear to be sexually better satisfied than those on the upper levels. The sexual techniques advised by marriage counselors and manuals are upper class ideas; the authors suggest that they may be not merely inapplicable, but an actual outrage to the mores of the lower levels. However, elsewhere the authors give evidence of downward diffusion of upper class mores, and we have still to hear about the practices and mores of the Negro, and *we hope* also, of some groups fresh from South European and Asiatic cultures.

The differences between the three religious faiths are much less than the differences between the religiously active and the religiously inactive. The religiously inactive, as we might expect, report more non-marital intercourse, and also significantly higher frequencies of marital intercourse! Here, of course, there might be some "cover-up."

To test the theories of mass change in sex behavior, the total sample was divided into an "older" and "younger" generation, born on the average about 1900 and about 1922 respectively. (Extremes of distribution were not compared.) Only minor differences were found. Incidence increased and age of beginning the activity became younger in the masturbation, nocturnal emissions, and petting experience of the grade school level, and in petting experience and petting to climax in the college level. The only change in intercourse is that it begins at somewhat earlier ages, in the grade school level only.

The authors see in these changes only an earlier maturing, probably due to better nutrition, in the lower levels, and a greater permissiveness toward petting, starting in the college level and filtering down somewhat to lower classes. However the alleged sexual revolution is supposed to have taken place during and before the 1920s and to most of Kinsey's "older generation" it would have been a *fait accompli*; there are few data on the Victorian males born around 1870. The authors reject the Terman findings as based upon heterogeneous and inadequate samples.

However, a more significant, and allegedly still-continuing sexual change lies in the number and types of women involved in sexual contacts. For conclusive evidence as to this, the forthcoming report on the female must be awaited. There are some adumbrations, however, in the present book. At each social level the percentage of males who contact prostitutes is about the same in the "younger" as in the "older" generation. However the frequency of these contacts has been reduced to two-thirds or a half as much as before. The writers also record their impression that the number of girls engaged in prostitution is not materially less than ten or twenty years ago but that these operate more as individuals and make less frequent contacts. Prostitutes, they estimate, supply something between 8 and 15 per cent of all extra-marital intercourse (not including pre-marital).

So far, at least, the result of the study seems not so much to reveal trends but to establish more convincingly the large and perhaps longstanding discrepancy between actual sex behavior and the publicly expressed mores or apparent themes of American culture. The authors carefully refrain from making moral judgments. However they make us wonder if we know what the real mores are. We get glimpses of "grass-roots," operating mores, differing from book and platform mores, and varying greatly among social groups, sometimes leading to tragedies where two incompatible codes conflict.

Of course many will question the honesty and memory of the subjects and the possible selective influence of the volunteer recruitment. The authors recognize these problems and estimate certain possible errors. Within the interview there are skilful checking devices. Some groups were covered 100 per cent, and there are many internal consistencies as between comparable groups, and as between samples of different degrees of coverage. There are strong consistencies in retakes and between different interviewers. It is to be hoped that some samples of both

women and men will be rechecked by women interviewers.

To provide an adequate view of modern sex life, this statistical analysis of frequencies and incidences needs to be supplemented by another type of analysis, which cultural anthropologists should help to plan. We need to know more about the contexts, the situations which stimulate and inhibit sex behavior—including the erotic excitements which do not lead to orgasm. How are these sexually meaningful situations changing? Are there *any* groups of females, or of other males, which have the wide range of erotic conditionings of the upper class male? Are there communities, social cliques, ethnic groups, which have distinctive sex cultures which are obscured by the census and other standardized classifications of population? What are the sex patterns of persons fresh from non-American cultures? There are enough representatives of many of these now in the United States to permit useful study even without leaving our shores, as the anthropologists found in their wartime studies of character structures. What is the process of change in sex culture? What is the role of economic and of ideological factors? What roles are played by conversation, by concealment, by rivalry? What are the channels of communication *re* sex behavior? Is it true, as some have alleged, that lower class sex mores have filtered upward, or is this an illusion caused by looking at the mere frequency or permissiveness of premarital intercourse as if this were the whole, or the major part, of the sex culture? In view of the greater petting, intellectual eroticism, and prolongation of love-making, does sex take more time and play a larger role in the life of the more educated classes, regardless of "outlet" frequencies? How does sex behavior interact with the rest of life?

Future reports of the Kinsey group promise at least partial answers to some of these questions. This reviewer has had some contact with the group for some years, and it seems to make increasing use of these cultural points of view.

JOSEPH K. FOLSON

Vassar College

✓ *Can Science Save Us?* By GEORGE A. LUNDBERG. New York: Longmans, Green, and Co., 1947. 122 pp. \$1.00.

The central thesis of this book is that man's best (and only) chance to solve the social problems besetting him is to provide the conditions necessary for the large scale application of scientific methods to the study of human rela-

tions. The analysis develops three basic ideas:

- (1) that man's great need is a method of discovering efficient means for achieving his ends.
- (2) that the scientific study of human relations is not only possible but that it is "the most effective means so far discovered by man for whatever ends he chooses to pursue."
- (3) that the preconditions for the solution of man's social problems by science include social changes in education, in financial aid to research, and in the status of social scientists but that they do not include any one of the particular economic, political, or social systems among which man can choose today.

Most sociologists today will probably agree with Lundberg that a "science" of human relations is possible and that such a science is necessarily instrumental in character. However, this does not mean agreement that social science can "solve" man's pressing problems. In many areas of social life conflict seems to arise at least as much from disagreement on the ends to be achieved as from the absence of such efficient means as social science might provide. In the main, Lundberg avoids this problem by specifying that the scientist in his role as scientist does not choose the values but only finds the best way of implementing them. However, he does indicate at one point that "... there is general agreement . . . on the broad goals of life as evidenced by man's behavior . . . the sharp differences of opinion arise about the means, the costs, and the consequences of different courses of action. . . ." The goals of life on which there is agreement are specified in such "broad" terms as "physical survival," "security," "a livelihood for the individual and the group," and "recreation." It is true that much of the activity in every society can be classified from the researcher's point of view under such general headings, but this does not negate the fact that acute disagreements exist within many social units on the specific ends which are being sought within any of these general areas of "agreement." Part of the problem here is that Lundberg appears to define as "means" what may actually be "ends" of the most sacred value. Thus, he defines social organization as "merely a means to the ends that men seek." It is quite clear, however, that in most societies at least some aspects of the social organization are cherished ends for which men will make sacrifices and die, if need be.

The means-end problem enters also into an evaluation of Lundberg's proposals for revamp-

ing the educational system. He proposes that the curriculum be centered about (1) science and the scientific method, (2) the arts of communication (including mathematics), and (3) the arts, with science and the scientific method as the integrating core. This reviewer is heartily in agreement with Lundberg's proposal that the scientific method be given a larger importance in the curriculum. However, there may be a serious question as to whether society can "save itself" with an educational curriculum stressing the techniques and findings of science without an important emphasis also on helping the student to develop a unified and consistent set of values for the "good life." It may be that Lundberg contemplates that this latter function will be fulfilled under the "arts" which he defines as "... those subjects and behaviors to which men have always looked for refreshment, release, creativeness, and pleasure. . . ."

One persistent theme of the analysis with which very many social scientists will disagree is that because of their instrumental character the social sciences, like the natural sciences, *could* function equally well under a wide variety of political-economic systems, that, for example, "democracy" has no special advantage in this respect as compared with "communism" and "fascism." This "objective" point of view fails to recognize the relationship between intellectual work and the forms of social organization, which Lundberg recognizes elsewhere in this book and in his other writings. In particular this view involves the false assumption that the basic unity of social and physical sciences with respect to methodological framework means that the differences in subject matter will not affect the kinds of social orders under which the social sciences, as distinguished from the physical sciences, can function most effectively. For example, a political system of control which limits research or the free exchange of information with respect to human relationships for which the "correct" views are prescribed by the state is hardly the most favorable for the social sciences. Lundberg's development of the idea that all the sciences have prospered and declined under all kinds of social orders is not convincing with respect to the social sciences. In this connection it is interesting to note that public opinion polling, which is considered by Lundberg to be an important strategic advance in the social sciences, was not developed in Germany or Japan to any extent despite a pressing need for such information. Skepticism concerning the enduring "sacred" value of any particular

form of government need not encompass the view that work in the social sciences cannot be more compatible with one or another political or economic form.

Many sociologists, including the reviewer, who are in agreement with Lundberg on the basic methodology of the social sciences, will not agree with him concerning its implications for the social role of the sociologist or his illustrative substantive statements on such matters as our foreign policy. The reviewer believes that it would be unfortunate if the merits of the basic methodological position were to be obscured by connection with a set of ideas and substantive conclusions which are not necessary to the methodological position and about which sociologists are not in agreement.

The reviewer recommends the book for students of social research as a lucid and stimulating statement of an important position. Like many other pleas for a dispassionate social science, it is written with considerable passion.

RONALD FREEDMAN

University of Michigan

- ✓ *Medicine and the Changing Order: Report of the New York Academy of Medicine, Committee on Medicine and the Changing Order.* New York: Commonwealth Fund, 1947. 240 pp. \$2.00

The Academy report under review is best appraised in the perspectives of the long controversy of which it is a part. It is now a score of years since serious studies have been made and proposals formulated on methods of coping with the important problem of the high costs of medical care, with a view to providing adequate medical services of high quality to all segments of the American population. In November, 1932, the *Final Report of the Committee on the Costs of Medical Care* was published after five years of research by an authoritative group of experts, under the chairmanship of Ray Lyman Wilbur. This report was furiously denounced as "socialism and Communism—inciting to revolution" by the organized medical profession. The American Medical Association was not in the vanguard either in initiating or supporting reforms in the distribution of medical care, but on the contrary took a narrow guild approach to the problem. It is noteworthy that the large majority of the Committee on Costs of Medical Care merely advocated, in that final report, group practice and health centers, and preferred voluntary to compulsory health insurance (although recognizing the limitations of

the former) on the ground that "the ultimate results will be far better if experience with actuarial and administrative details, and above all the evolution of group practice units capable of rendering rounded medical services of high quality, precede the adoption of any compulsory plan by any state as a whole."

Important innovations followed in the ensuing years. During the depression the straitened finances of local and state governments made it imperative that the federal government subsidize various types of medical services. Then came the passage in 1935 of the Social Security Act which provided grants in aid to States for public health departments and for infant and maternity health services. On the basis of the findings of the extensive National Health Survey made in 1935-36, the first Wagner Bill was drawn up which proposed a system of grants-in-aid to states for the extension of medical services. The representatives of the American Medical Association trained their propaganda guns against such grants-in-aid, characterizing federal aid of any sort as bureaucratic interference with the prerogatives of the medical profession. The proponents of a national health program then drafted the Wagner-Murray-Dingell Bill which, in its several versions, proposed a comprehensive national health insurance plan along with grants-in-aid from federal tax funds. During and after World War II the federal government entered further into the health scene through the Emergency Maternity and Infant Care Program for the wives and infants of soldiers, and later by an enlarged medical program for veterans, and grants-in-aid for a hospital construction program. The popular support from labor, consumer and civic organizations which these programs and the Wagner-Murray-Dingell bill received, and the widespread manifestation of hostile sentiment which the opposition of the American Medical Association evoked, led to a reluctant retreat and some changes in tactics on the part of the organized profession. Opposition to the principle of federal grants-in-aid was dropped and grants for the medical care of the indigent, as under the Taft bill, and for hospital construction were approved.

The focus of the attack came to center upon compulsory health insurance and particularly upon a national health insurance system. In order to meet the criticism of persistent obstructionism, the American Medical Association has offered as its positive program, voluntary health insurance plans under the control of the local

and state medical societies. After its defeat in courts in its efforts to interfere with local voluntary plans such as that of the Group Health Association in Washington, D.C., the A.M.A. has abandoned such overt tactics. Indirect methods, however, are still being used to forestall the operation of voluntary groups not administered by the medical societies or according to specific stipulations which they have laid down. Group practice, previously rejected on the ground that it ousted the general practitioner from his rightful place as the central agent in medical practice, is now formally approved.

When the distinguished New York Academy of Medicine established its Committee on Medicine and the Changing Order, many persons felt that it might go considerably beyond the official policy of the A.M.A. in offering guidance to perplexed physicians and to the public, both of which need such guidance. For costs of medical care have continued to mount and medical services of high quality are beyond the reach of the low income groups. Hospitals have increasingly become the strategic centers of medical education, research and therapy, transforming the structure of medical practice. Advances in medical science have led to greater specialization, have lengthened the span and increased the costs of medical education, and have complicated the relationships between specialists and practitioners. Physicians continue to migrate to the cities, leaving rural areas medically impoverished. State and municipal as well as federal government medical services have expanded their scope. Voluntary medical insurance has grown both in variety of programs offered and in number of persons covered.

Several monographs have previously been published by the Academy committee that have indicated the nature of these and other trends and have suggested their implications. They refrained, however, from offering programs, for this was conceived to be the function of the final report now under review. The report is dignified and decorous in style of presentation, in sharp contrast to the polemics recently widely circulated by the National Physicians Committee, the propaganda arm of the A.M.A. It contains many enlightening proposals, particularly those concerned with the expansion of public health services, based on the valid thesis that the best way to reduce medical costs is to prevent illness. The Committee puts itself on record against all discrimination in terms of sex, race, or creed, and against economic discrimination in the selection of medical students. It supports group

practice unequivocally. It asserts repeatedly that subsidies are necessary from city, state and federal tax funds to improve the distribution of medical care. Many other of its proposals will strengthen the constructive forces within the medical profession.

However, on the crucial issue of health insurance the report does not go beyond what is now the official stand of the leadership of the A.M.A. The entire discussion of health insurance is fragmentary, shallow and inconclusive, and displays a lack of knowledge or a neglect of important recent literature in the field. Its categorical dismissal of compulsory health insurance, with the very vague qualification "at this time," is based on the judgment of a few selected European opponents of the plan. Out of keeping with the judicious tone of the remainder of the report, this discussion is particularly subject to criticism because nowhere in the earlier monographs of the Academy have the pros and cons of compulsory health insurance been presented and analyzed. Particularly inexplicable is the fact that while a preliminary statement of the findings of the Committee made by the president of the Academy, Dr. George Baehr, and published in the *Journal of the American Medical Association* on January 12, 1947, contains the proposal that compulsory insurance be tried out on a state basis, the final report is completely silent on this possibility. The analysis of the nature of voluntary plans is deplorably sketchy. It evades some of the basic issues in the controversies over their functioning, such as whether there should be public representatives in the administration of the plans or whether they should be entirely under control of organized medicine as is now required by the statutes of many states.

The conclusions of the Committee report are thus strangely reminiscent of, yet perhaps even less advanced, than those of the Committee on Costs of Medical Care in 1932 cited above. There is commendable frankness in admitting the limitations of the voluntary plans as when the report declares that "few plans provide anything like complete medical coverage for the entire family, and experience has shown that, even when it is offered, most people are unwilling or unable to pay the high premiums required to provide prepaid, comprehensive medical care." No solution is proposed to meet these difficulties except perhaps the audacious and ambiguous proposal that the federal government subsidize voluntary plans. No analysis is made of the administrative role of the government should

such grants-in-aid be made, for the government could not disburse tax funds without some sort of supervision over their expenditure. In the face of the urgent need for positive action to improve the distribution of medical care among low income and rural groups all the Committee offers is the judgment that "It conceives voluntary insurance as an essential experiment in prepayment, which avoids the pitfalls of compulsory insurance." At this late date, such general advice for experimentation is hardly enough.

BERNHARD J. STERN

Columbia University

○ *An Evaluation of Non-Directive Psychotherapy by Means of the Rorschach and Other Indices.*

By GEORGE A. MUNCH. Applied Psychology Monograph No. 13, American Psychological Association, California: Stanford University Press, July, 1947. 163 pp. No price indicated.

This monograph is essentially based upon three tests, the Rorschach, the Kent-Rosanoff Word Association Test and the Bell Adjustment Inventory, given to 12 clients of the Psychological Clinic at Ohio State University before and after a period of non-directive psychotherapy.

Of the original group of 15 clients selected at random, three failed to continue treatment. Twelve were carried in treatment by different therapists for varying periods. Some problems were acute reactions precipitated by recent frustration and it would seem, might have been expected to improve without treatment. Others were more long-standing.

The Rorschach performance was interpreted to indicate favorable progress in 11 of 12 cases, the Kent-Rosanoff in nine of 11 cases, and the Bell Adjustment Inventory in seven of 12 cases. It was possible to demonstrate a tendency for an improvement in performance on these tests, and the indications of improvement tended to correspond with the therapists' estimate of the presence and degree of success in treatment. (The therapists considered four cases successful, eight less successful.)

The author wins respect as a careful, conscientious worker, but it is difficult to say what he has accomplished in this study beyond finding interesting indications of the possibility of utilizing of such tests in the evaluation of progress in therapy.

The study illustrates the current tendency of clinical psychologists to fall into some errors psychiatrists are gradually learning to avoid, as

well as to commit others to which psychiatrists have never been prone.

There has been a repetitive tendency for various workers in therapy each to develop a system adapted to his own personality and applicable to some cases, and then to seek to crystallize and teach his usual practices as the whole of therapy. This results effectively in the establishment of a rather rigid concept of therapy and an inevitable if unrecognized focus upon technique rather than upon process. In effect it becomes more important to follow the right technique than to help the patient recover, for if he fails to recover with the right technique, that is his responsibility, but if he recovers with a wrong technique, the treatment is superficial and the therapist is a charlatan.

It has taken psychiatry nearly two generations to begin to get over the divisiveness of rigid schools of therapy, and to reduce the defensive rationalizations developed to discount the success of heretics. Clinical psychology now appears to be embarking on a similar crystallization of therapy. Even the author feels he must defend his heretical use of tests before non-directive therapy.

The author mistrusts the clients and the therapists' estimate of improvement in therapy, and with a scientists' passion for objectivity seeks to substitute objective measures. However the measures to which he turns are either of unestablished validity for his purpose (Rorschach and Kent-Rosanoff) or else represent mainly a standardized subjectivity (Bell Inventory) as he himself admits. He discards as not feasible any objective inquiry into the client's actual change in adjustment. In defending his choice of instruments presumed to deal with the structure of personality, he seems to dissociate adjustment from personality structure to a degree which the reviewer finds hard to follow. His thinking seems however to echo the traditional defense of the partisan psychiatrist that improvement effected without the use of his method of treatment cannot possibly be cured and must therefore be superficial. Mistrusting the report of client and therapist and considering objective study of adjustment as not practical, he turns to objective methods not validated.

The usefulness of this exploration is impossible to state at present, depending as it does upon future work. The reviewer cannot accept it as an evaluation of non-directive psychotherapy, as the title claims.

R. L. JENKINS

University of Illinois

The Psychology of Ego-Involvements. By MUZAFER SHERIF and HADLEY CANTRIL. New York: John Wiley and Sons, 1947. Pp. viii + 525. \$6.00.

One of the more persistent weeds in the social science garden is Freudianism. It is an adaptable plant, thriving without factual nourishment, that returns, season after season, in one or another of its apparently infinite variations. Many have tried to hoe it out, many have tried to smother it with the preponderance of contrary evidence, and some have endeavored to kill it with poisonous words. But so far the species has survived, and at the moment it is again flourishing at the expense of more useful and productive constructs.

In *The Psychology of Ego-Involvements* Sherif and Cantril attempt to dispose of Freudianism in social psychology by a new technique, that of destroying by reinterpretation. The result is a long, involved, repetitious, and tiresome discussion of what "ego" really is, where it comes from, and what it does. The discussion is replete with quotations from authorities, mainly anti-Freudian, of course, and with references to the findings of the experimental psychologists. Those who have come to social psychology from sociology and have brought with them the basic concepts of Cooley and Mead will find little objectionable and nothing novel or interesting in the book. Those who persist in polluting social psychology with Freudian mysticism will do well to read it and may even profit by it.

For these latter let it be said that Sherif and Cantril use the term "ego" with reluctance and then proceed to demonstrate with argument and evidence that ego is not an entity, as the Freudians hold, but an aggregation of many specific attitudes "which from infancy on are related to the delimited, differentiated and accumulating 'I,' 'me,' 'mine' experiences." (p. 4) As Sherif and Cantril use it, the term "ego" is thus practically indistinguishable from that of "self" as used by Cooley forty-five years ago and by Mead not long after.

Now, not only is the ego not an entity, but it is not, as the Freudians persist in believing, something that is established at conception to unfold, in strange and wondrous ways, throughout the life of the individual. Ego is, on the contrary, acquired out of experience, mainly social, and its characteristics are therefore dependent upon time and place. To the elaboration and demonstration of this thesis, Sherif and Cantril devote a number of chapters, one of which, "The Genetic Formation of the Ego,"

is a rough and awkward approximation of the chapter entitled "Sociability and Personal Ideas" in Cooley's *Human Nature and the Social Order*.

Moreover, they contend, the ego is not something that, once developed, remains constant throughout the life of the individual. It is more or less dynamic, changing, perhaps belatedly, to meet changing social status and conditions. Its dynamic character is most clearly evidenced at adolescence, when in our society it is rather rudely shocked by new and unprecedented social demands (Chapters 8 and 9), and during times of war and revolution, when breakdowns and occasional re-formations may occur (Chapter 12). Anyway, Sherif and Cantril present ample evidence to show that the ego is never static. All of this should be very interesting to those who thought it was; and perhaps it is merely carping to point out that Cooley discussed the matter at some length and much more clearly long, long ago (*Human Nature and the Social Order*, Chapter V). But that was before the Freudian invasion, so perhaps it does not count.

Well, finally, Sherif and Cantril arrive at the point that the ego, a somewhat dynamic product of social experience, does not operate in a vacuum. It is not something that will everywhere and inevitably "express" itself, as the Freudians contend. It operates only as one complex factor in a field of forces; that is, the individual behaves mainly in and as a constituent member of social groups. The term "ego" is consequently incomplete. The ego is unstable in that the particular attitudinal component of the ego that will manifest itself will depend upon the situation obtaining; as the situation changes, so too will the ego manifestation. To stress the fact that the ego never operates and is never known apart from some sort of situation, the authors have resorted to the title term "ego-involvements." To demonstrate this now well-established fact, they have written two marvelously involved chapters.

Comparison with the work of Cooley grows as tiresome as the book itself, but comparison may in this instance clarify. What Sherif and Cantril mean by ego-involvement turns out to be nothing more than what Cooley meant by "the social self" and what Mead discussed, less clearly but at greater length, under the term "self-other." Little has been added but obscurity. But the blame lies not with the authors of the present book, who are endeavoring to render modern social psychology a real service. The blame lies with those wishful, muddy thinkers

who turn at the slightest frustration from the search for verifiable fact to the dubious authority of Freud. To combat such thinking, it is perhaps necessary to reiterate in the language to which such thinkers are accustomed what should by now be clearly evident.

RICHARD T. LAPIERE

Stanford University

- *American Military Government in Germany.* By HAROLD ZINK. New York: The Macmillan Company, 1947. 272 pp. \$4.00

Professor Zink is either an excellent chess player or has all the qualifications to become one. His book reveals a degree of zest in the tracing of complicated moves and counter-moves, and an amount of expository skill in rendering these moves intelligible, which would do credit to an Alekhine.

The story he tells reaches only to the beginning of 1947, and consequently is sadly outdated in many respects. Nevertheless, enough stability had been achieved by that time to make large sections of the work useful as a guide to Military Government even now (January, 1948).

The book is both historical and topically analytic. There is heavy emphasis on the structural aspects of military government, as might be expected of a political scientist with a strong interest in public administration. The numerous charts reinforce the structural emphasis, although they add little to the text itself. The strength of the book lies in the obviously wide experience and professional equipment of its author; the weakness in failure to establish priorities amid the wealth of fascinating detail. The interested reader, however, can readily derive salient points from the occasionally indiscriminate recital if he exercises a little patience.

Apart from the outlines of MG structure, one of the things that emerges most clearly from the details of Zink's book, even though he nowhere directly stresses the point and on occasion implies the contrary, is the amazing inability of the American Army (and that part of the American government submissive to the Army) to do effective planning and administration. The indecision, improvisation, irresponsible shifts of policy, personal and group vanity, corruption, and plain ignorance in high circles which made "snafu" and "fubar" terms of hourly use during wartime would be incredible if the evidence were not all too abundant. It seems probable that had we been pitted against a Germany equal in numbers and equipment and free of Nazi inter-

ference with major strategy, crushing defeat would have been our lot. Why? Because in spite of the superiority of the individual American soldier and his *matériel*, the Regular Army officer, particularly at higher levels, was all too often a conceited incompetent. With a few striking exceptions such as Eisenhower, Patch, Bradley, Huebener, and Clay, our best brains stayed away from a martial career, and it needed all the resources of the RAPA (Regular Army Protective Association) to shield mediocrity from exposure.

Only as MG has slowly shaken off the handicaps resulting from domination by Army unteachables has it begun to achieve even its present limited degree of success. Manifold shortcomings still remain, but the contrast between MG as it was three years ago and its present makeup and functioning offer some ground for encouragement.

The second main point which can be elicited from Zink's cautiously presented details is the devastating effect, viewed in the light of *American* interests, of the Morgenthau plan. Why anyone as perspicacious as the late President Roosevelt should have permitted the amateurish judgments of the Secretary of the Treasury to override the conclusions of scores of qualified experts in the State Department and elsewhere is still something very close to a mystery. The almost fatal delay in issuing the *Handbook for Military Government in Germany* is traceable to this source alone, and it is good to see that Zink has had the courage to lay the blame at the proper door.

A third major point to which Zink devotes a full chapter and a considerable amount of incidental comment in other parts of the book is the initially niggardly support given to the program of re-education. Matters have slowly improved, but it was only in October, 1947, that an educational adviser coordinate with other members of General Clay's immediate group of counsellors was appointed. Even with recent gains in status for the combined unit known as Education and Religious Affairs, it nowhere enjoys the rank of a division except in Hesse. At the Berlin level it is a pitifully understaffed branch of the Internal Affairs and Communications Division, which gives shelter to four or five other branches ranging from public health to postal service.

In more or less arbitrarily singling out these points a certain injustice has been done to Zink, for his book is systematically organized and abundantly factual. At a dozen different places

where the reviewer has been able to check directly, as for example the episode of the ineffable Colonel K—in the early days of MG in Bavaria, or MG difficulties with Patton, Zink is clearly writing from accurate inside information. His conclusions, marked by a qualified optimism sometimes inconsistent with his earlier presentation, are in general acceptable, although few Americans now in the Berlin district would speak so favorably of Russian cooperation. The final chapter seems thoroughly sound, and the advice for the future, particularly with regard to the staffing of an American civil administration for Germany, is eminently realistic.

American Military Government in Germany deserves wide circulation, not only among those most directly concerned but also among American citizens who realize that we have a bear by the tail and can't let go.

HOWARD BECKER

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(on leave, University of Wisconsin)

Public Reaction to the Atomic Bomb and World Affairs: A Nationwide Survey of Attitudes and Information. By the Social Science Research Council, Sub-Committee on Public Relations. Ithaca, N.Y.: Cornell University, April, 1947. 310 pp. No price indicated.

For some time, I have been complaining that the preoccupation of opinion researchers with the problems of sampling large publics in the period since the pollsters became active in 1935 had decreased the amount of attention and study of the methods for intensive recording of attitudes and their expression in opinions. Recently, some swing back to the central problem of the intensive attitude study may be noted. This study of public reaction to the atomic bomb is a combination of both approaches. A report based on the extensive polling of 3,090 people before Bikini and 2,894 people after Bikini is accompanied by a report based on interviews with 600 subjects before and the same number after the bomb tests. The first report has been prepared by Richard S. Crutchfield of Swarthmore College, and the findings of the intensive surveys are summarized by Angus Campbell, Sylvia Eberhart and Patricia Woodward of the Survey Research Center of the University of Michigan.

This study was directed by a subcommittee of the Social Science Research Council. The committee members were: Hadley Cantril, Pendleton Herring, Rensis Likert, and Leonard S. Cottrell, Jr., Chairman. The surveys cost

over \$47,000, half of which was granted by the Carnegie Corporation of New York and half by the Rockefeller Foundation. Opinion and attitude polling and study is expensive. This is the principal reason why subsidized, non-commercial studies have not been widely developed as yet. One could not be extensively voluble in question asking for, say, a million dollars a year. But, such subsidization must be forthcoming, if studies which the commercial pollsters cannot afford are to be carried on. This is one of the pioneer studies.

Part I of this Report consists of a few pages of explanation and summary and about 50 pages of tables based on the extensive polling survey of 6,000 subjects. Standard polling methods were used, with the interviewer recording statements of opinion and factual answers, and handing the respondent a card with four or five multiple-choice statements on a few of the questions. The survey sheets contain about 25 questions (two forms were used which differ slightly from one another), of which ten inquire about information and opinion on the atomic bomb and atomic policy, and a number of others are questions concerning the economic status, education, and political affiliation of the respondent. Five questions on general information were scattered among the others on the questionnaire. After years of noting the low level of popular information as evidenced by poll results, I must admit that I was somewhat surprised to learn that only 41% could correctly name the U. S. Secretary of State. But, too much can be made of the lack of specific information. As Charles Horton Cooley wrote, "The masses preserve the sentiments," and a person quite uninformed as to the name of the U. S. Secretary of State or the official position of General Leslie R. Groves or the materials from which atomic energy is now made may have some meaningful sentiments about the use of the bomb.

Although the polls reported in Part I appear to have been conducted with professional competence, it is the more intensive survey reported in Part II which is methodologically more interesting and, due to the variety of questions, more informative for the reader. Each sample consists of approximately 600 adults. Your reviewer, not an expert statistician, remains an unreconstructed skeptic concerning these very small samples as a cross-section of the United States. Aside from the size of the sample, the sampling methods would appear to be sound insofar as one can tell from the report. In such studies, how-

ever, in order to be certain of details, one needs to have taken part in the study or trust the competence of the directors and staff. No report is ever detailed enough. In this report, I have confidence in the directors.

The interviewer filled in an information sheet about the respondent's sex, age, education, income, political and religious preferences, and a number of questions on his sources of information, and then, after a number of broad appraisal questions to get the interview started, he began on the 43 questions of the survey. Extended, guided discussion ensued. The interviews are referred to as lengthy, as indeed they would have had to be to cover the variety of topics. I should like to have known the average length of the interviews, but do not find such a statement in the report. Seven questions dealt directly with the atomic bomb, 3 with the bomb's effect on the size of the armed forces, 3 on the Bikini test, 5 on sources of information about the bomb, 5 on the role of the U. S. in world affairs, 4 on England, 4 on Russia, 8 on the United Nations, and 3 on world organization. Even the general results are too extensive to mention here.

Of course, all specialists in the field of public opinion study will examine this report. I should recommend that all social scientists peruse it for its content interest. It would appear to be the most extensive and most competently administered survey conducted thus far on any single topic.

WILLIAM ALBIG

University of Illinois

• *The Problem of Reducing Vulnerability to Atomic Bombs.* By ANSLEY J. COALE. New Jersey: Princeton University Press, 1947. 116 pp. \$2.00

As noted in the preface, this book does not attempt to answer the problem of what should be done towards reducing vulnerability to atomic bombs. Mr. Coale's most comprehensive study is a cogent thought-provoker directed toward bringing all readers, specialists as well as laymen, to a common ground from which they may attack the problem. The author, eminently qualified to present such a treatise, is not overly optimistic; he is most blunt in pointing out the many complexities and difficulties that interpose themselves in the consideration of various avenues of attack. But, fortunately, he indicates that it is not altogether impossible to devise practicable methods by which mankind

can substantially reduce its vulnerability to atomic warfare. This is in extreme contrast to the more commonly accepted defeatist viewpoint which maintains that the only protection is either the "big stick" method or the outlawing of atomic warfare altogether by international controls, a procedure which most will agree our twentieth century civilization seems not yet capable of effecting.

Human nature being what it is, there is no assurance that atomic bombs will not be directed toward strategic targets in our country some day. The need, therefore, for making adequate preparations for such an eventuality is incontestable. Since the development of counter-measures against the latest forms of warfare is in its infancy, complacency or waiting for the day of their ascendancy over the weapon itself is out of the question. This day may, in fact, never arrive for, as Mr. Coale's closing theme states, unless some effective restraints can be imposed on military techniques, we may never attain protection against newly-developed forms of warfare.

The problem of reducing vulnerability is presented under two assumptions: first, that effective international limitation of atomic weapons can be effected and, secondly, that atomic armament is unlimited. Under each he discusses the part adequate precautionary measures would play in preventing an attack, insuring against the loss of a war, minimizing casualties and damage, and aiding reconstruction. The latter part of the book summarizes some technical facts assumed earlier regarding the characteristics, methods of delivery, and possibilities for defense against the atomic bomb, suggests topics where research is needed and discusses some of the obstacles to be encountered in formulating a program.

Although any contemplated program "must not exceed the bounds imposed by public and congressional opinion," there exists a very great danger of hitting upon either side of a well-balanced, thoroughly thought-out middle course. The two extremes are a continued complacency or lack of full appreciation of the necessity for immediate initiation of protective measures on the one hand, and on the other, an all-out program for the development of subterranean living and industry wherein the fear-driven populace enshrouds itself with a false feeling of security and bases its preparedness solely on another Maginot Line of defense. Much remains to be done in securing public and congressional

support for any measure and, if Mr. Coale's book serves to crystallize public opinion on the subject, the author should feel amply rewarded.

THOMAS D. GILLIS,
Lieutenant Colonel, U.S.A.

Fortress Monroe, Va.

- *The Cultural Approach—Another Way in International Relations.* By RUTH E. MCMURRY and MUNA LEE. Chapel Hill: The University of North Carolina Press, 1947. xiv + 280 pp. \$3.50

The popular use of the term "cultural approach" in the title of this book should not distract the sociologist or anthropologist from realizing the vitally important field for sociocultural research implicitly opened up by the volume's contents. If there is one field of international and inter-people relations which should be worked practically by government and scientifically by research forces, it is this one. The failure of the two to cooperate heretofore can only be attributed to the inexcusable conservatism and naïveté of the U. S. State Department on the one hand and the equally inexcusable faults of social scientists on the other hand.

The authors are not advocating such cooperation. Theirs is the initial job of publicizing the nature, extent, and practical importance of the field. The propagandic promotion abroad of one's national Kultur had been regarded in this country as primarily if not purely a tool of outdated imperialism. It had left such an odoriferous trail behind it that the "public," Congress, and social scientists generally have been even slower than the State Department to grasp its wider import. Indeed, that such "intellectual expansion" as that of France during the latter half of the nineteenth century, might not have been any more imperialistic or nationalistic than our own current program of cultural relations, hardly occurred to most thoughtful citizens or social scientists of the United States. That, at present, "it would be difficult to mention a land with a national consciousness which does not have a program of cultural relations abroad," (p. 238) may be just as surprising to most of them. But whatever their past notions of such relations may be, it is imperative that they now become aware of the world-wide realization among governments that in this field lie most formidable "weapons" which may decide the fate of nations in war or in peace. (p. 230)

If the reader questions this contention of the

two participant authors and of Archibald MacLeish who writes the Introduction, the least he can do is to read the successive accounts of France's "Intellectual Expansion," Germany's "Preservation of Germanism," Japan's "Racial Approach," the U.S.S.R.'s "Cultural Approach," Great Britain's "National Interpretation," Four Latin American Republics' spread of their "Cultural Progress," and the program of the "Late-comer in the Field," the United States. Not to be overlooked are the foreboding hints of the very real and explosive competition if not conflict in the field, and the hopes that UNESCO may iron out effective rules of the game and itself promote a more impartial universal type of international and inter-people understanding.

From the additional standpoint of culture contact, conflict and adjustment, the reviewer would point out that the glaring need is, first, a broad scientific orientation for the entire field; then, a number of intensive sample studies of the effects of different kinds of "cultural relations" activity in specific culture areas under defined conditions; third, the initiation of a broad program of studies to keep a constant check on the effects of the activity in general. As the writer is all too poignantly aware from his own studies in the field of culture contact over a period of 25 years, that undertaking would be an enormous one. But, if this or any country is to be intelligent as to what it is actually doing in this vital field of international and inter-people relations, is there any alternative?

We are indebted to the authors for a new, well-documented, interesting introduction to the activities and aims of leading world powers in the field. The next step should be ours.

MAURICE T. PRICE

Wayne University

- *Race and Nationality as Factors in American Life.* By HENRY PRATT FAIRCCHILD. New York: The Ronald Press Company, 1947. vii + 216 pp. \$3.00

Here is a book which makes a contribution to the difficult task of stimulating thought and developing social intelligence concerning one of the most complicated and perhaps most difficult social problems facing the modern world. At the risk of the scientific fallacies involved in oversimplification it attempts to set forth fundamentals basic to an understanding of race and nationality and to appraise these factors for sound and constructive action in social affairs.

Sociologists are familiar with the point of view represented in this volume. Race is explained as a strictly biological concept and nationality is viewed as a purely cultural concept. Five of the 13 chapters composing the book are concerned with an elaboration of the nature and origin of racial and nationality groups. The layman, for whom this work is primarily written, will probably experience little difficulty in grasping Professor Fairchild's conception of the nature and origin of race, but his treatment of nationality apparently lacks the clarity it was designed to give. Nationality is conceived to be a "group of people that are characterized and distinguished by a cultural pattern which embodies certain definite features" (p. 36). Again, it refers to "uniformities in accepted and standardized ways of providing for human needs, of every degree of importance" (p. 36). Further, a nationality "is essentially a spiritual reality" (p. 37). Then, it may be viewed as "a group of people who feel alike and together about a considerable number of the major interests and values of life" (p. 40). At still another point a nationality is, by implication, conceived to be a politically autonomous group. In any case, language, religion, and family system are considered highly significant aspects of a given nationality. In spite of the apparent variety in shades of meaning for the concept nationality, the reviewer is entirely in accord with the implicit assumption throughout the book that confusion and misunderstanding results from failure to properly distinguish race and nationality.

Race controversy is characterized as an issue highly supercharged with emotion resulting in many distortions of truth and logic. This leads the author to state that "if progress is to be made in the broad field of group relations one of the first and indispensable steps is to persuade people to stop talking, or even thinking, about general group superiority, whether racial or nationalistic. And if people stop talking and thinking about that kind of gradation it will follow as the night the day that they will also stop acting in accordance with such notions" (p. 85). Does this mean that race controversy is a war of intellectual nerves?

Four chapters of this book constitute a brief but stimulating discussion of the views known as Anti-Racism, the role of social values in the appraisal of group policies, the Americanization Movement, the nature of Anti-Semitism, and the Negro problem. Analyses in these areas are predicated on three assumptions: (1) national

integrity and solidarity are valuable assets to society and humanity, (2) race and nationality problems are separate and distinct, and (3) there is a fundamental difference between "prejudice" and "antipathy." The latter is very effectively demonstrated in the discussion of the Jews and of the Negroes. In the opinion of the reviewer, these assumptions are essentially sound, but sociologists will probably find much for debate in the author's use of them for analyzing minority group problems.

Perhaps the section of this little volume which will elicit the most interest and reaction from the layman is Chapter 12, entitled "What To Do." Professor Fairchild insists that the will to dominate and the dislike of one group for another identified by race, nationality, religion, size, and the like cannot be eradicated through threats, exhortations, legislation, or any form of coercion. The limitations of coercive effort are emphasized with the view to pointing up the issue of group values in a context of social policy based on social expediency versus social policy based upon material selfishness and the desire for individual and group advancement. No formulae for the solution of race problems are offered. The reviewer is not entirely in accord with the conclusion that "any constructive and enduring solution of race and nationality problems must rest on an essentially religious base—not the formal, institutionalized religion that bristles with dogmas, creeds, doctrines, and theological postulates, but the universal, fundamental, all-embracing religion based upon that world brotherhood and love of humanity that is professed and proclaimed by many of the great religions of today and practiced by none" (pp. 205-206). Such a conclusion begs the question of what to do. Much more realistic is Professor Fairchild's assertion that "the achievement of inter-group harmony, and the elimination of friction, must rest upon either the eradication of antipathy, or the control of antipathy and the suppression of its overt manifestations" (p. 206).

HENRY L. ANDREWS

University of Alabama

① *Hawaiian Americans: An Account of the Mingling of Japanese, Chinese, Polynesian, and American Culture.* By EDWIN G. BURROWS. New Haven, Conn.: Yale University Press, 1947. 228 pp. \$3.00

Those of us who have been encouraged by various writings to think of Hawaii as a Paradise of ideal race relations will get something

of a jolt from this book. True, it portrays, with much sociological insight, the practical adjustment of a variety of cultural patterns, but it also reveals that this adjustment has been reached through the medium of recognizing one particular culture, that of the *haoles*, or Europeans and Americans of European descent, as superior. *Haole* prestige is the leitmotif which dominates the whole drama.

The treatment is admittedly brief and condensed, but it is not superficial. It begins with a historical account of the coming of the white men to the Hawaiian Islands, and interestingly enough records that the early basis of the recognized superiority of the intruders was found in the light color of their skins. For the natives had regarded fair skin as a sign of nobility, not for any racial reasons but because those of any race who spend a life of leisure in the shade are likely to present a lesser degree of pigmentation than those whose work keeps them out in the sun. Hence, it was a case of "prestige at first sight."

The book traces the reactions of the various non-*haole* elements in the islands, particularly the Chinese, Japanese, and native Hawaiians, to the increasing domination of the foreigners, and shows pertinently how the different values recognized by the various elements affected the character and degree of their adaptation to the admitted *haole* superiority. These values included religion, language, food, dress, and familiarity with, and devotion to, traditional patterns of sex morality, family relationships and group loyalty.

We find the standard divergencies between the original immigrants and their native born children, with an incomplete assimilation on the part of the older generation and a restless, unattached, disharmonious psychical and social outlook of the younger. Thus it is said that the younger Chinese are either Christians or agnostics. This cleavage shows up particularly in the field of intermarriage, where the tensions sometimes lead to such extremes as suicide.

The second half of the book is devoted to an exposition of the devices used by the non-*haoles* to secure relief from *haole* dominance, among them the familiar processes of Aggression, Withdrawal, including religious reversion, and finally Cooperation.

It is significant that the Hawaiians themselves have paid for their political advances by positive economic losses. "The Hawaiians have never regained a satisfactory economic position. . . . Politically (they) have been much better off."

Western civilization is always more ready to share its political blessings with backward areas than to give them an even break economically.

HENRY PRATT FAIRCHILD

New York

Trinidad Village. By MELVILLE J. and FRANCES S. HERSKOVITS. New York: Alfred A. Knopf, 1947. 351 pp. \$4.75 (cloth).

Professor and Mrs. Herskovits bring to their study of the village of Toco, in Trinidad, the insight and knowledge acquired during their previous research focused on the Negro. Thoroughly familiar with West African, Dahomean culture, they have attacked problems of culture change which resulted from the transplantation of cultures from West Africa to various parts of the New World—Haiti, Dutch Guiana, Brazil, the United States, and now, Trinidad.

Always having an eye for African survivals in the Americas, the Herskovits learned of the existence of Shango worship in Trinidad while they were in Port-of-Spain en route home from Dutch Guiana in 1929. Ten years later, with the assistance of a grant from the Carnegie Corporation, they were able to return to do field work in Trinidad. The Negro community of Toco was selected for study as having the prerequisites desired—isolation and small size. While Toco was, in fact, remote from the capital, it was discovered that the worship of Shango was not present in Toco. Hence a separate study of Shango worship in Port-of-Spain appears as an appendix to the village study itself.

The analysis of the structure of Toco society is prefaced with a theoretical discussion so that the reader may place the cultural details in the authors' frame of reference. The Herskovits state that previous attempts to formulate the laws of the dynamics of culture change have had only limited validity and have not stood up as universal principles. Most social scientists would agree that the theory of evolution of societies through fixed stages and "group mind" hypotheses are passé. Some would be more loath to abandon the contention that material elements in a culture diffuse more readily than values and that rural and urban cultures have distinctively different qualities. Toco itself presents the qualities of a typical folk culture and very few of its African retentions are traits of material culture. Be that as it may, the authors' criticism of these theories is made only in passing and as an introduction to a statement of their own theoretical framework.

Their basic argument is that change is the result of two different kinds of drives—one, the constant forces operative in every culture to maintain the balance between stability and change, or to accelerate change in situations of intimate culture contact; and two, the unique historical sequences of events which give rise to the particular form a culture will take at any given moment in history. This is certainly a proposition which will find few antagonists. It makes explicit the fact that universals must be sought in social phenomena of a generic sort. The particular form of the culture traits and the specific sort of changes which occur will result not only from the conditions of contact—slavery in Trinidad—but from the peculiarities of the cultures themselves. This latter concept is highlighted by the authors in terms of “cultural focus: those aspects of the life of a people which holds the greatest interest for them.” The authors state, “Under contact . . . especially where pressures are applied to force change, resistance is greatest in the focal area; or, if resistance is futile, the psychological resilience afforded by the process of reinterpretation comes into play.”

The discussion of the effect of slavery on the position of women in the culture of Toco is outstanding. The significant place of women in New World Negro societies as the “principal exponents of the culture”—the essential bearers of tradition—is explained in terms of the fact that men’s activities in African Negro culture are centered around management of extended family and sib organization, political activities, and the direction of religious cults. It was precisely these institutions which were either suppressed or were impossible to maintain under conditions of slavery. This “emasculatation” of African culture, as the authors so aptly phrase it, is still evident in the more important cultural role of Toco women today. However, one is left somewhat confused as to how this condition supports the thesis of cultural retentions and reinterpretations reflecting cultural focus, from a male point of view. This would appear to be another of those generalizations which does not hold under some historical conditions.

The descriptive analysis of Toco culture begins with details concerning the cost of living and the sources of income. The economy is not one of self-subsistence but essentially one based on wages earned for work on large estates or the public roads. This source of income is augmented with family gardens when land is available to rent. The economic institutions show both Afri-

can and European influence. A mutual savings organization (*susu*), which is a definite African retention, occurs along with government old-age pensions.

The family organization is subjected to scrutiny, particularly the nature of the two types of families: “keepers,” or couples united without benefit of clergy, and couples married by the Church. The operation of the family system, the rites of birth and death, and the discussion of the role of religion are analyzed along traditional anthropological lines. While the legally proscribed sect of “Shouters” has a much smaller number of adherents than the more orthodox Anglican, Catholic, Baptist and Adventist congregations, the Shouter ritual is studied in most detail. The sect shows the greatest number of customs of African derivation and ably serves to give insight into Protestant Negro religious behavior in the New World.

The formal cultural description continues with inquiry into magic and divination, fraternal orders, and a very interesting discussion of the English legal court as an institution providing an avenue of self-expression for the Toco Negro. In the concluding analysis of the retentions and reinterpretations of African culture found in Trinidad, the Herskovits are in their favorite element. The similarities to recognized African practices are deftly indicated in this chapter and do not intrude into the preceding descriptive material.

Toco society is more like that of North America than that of Haiti or Guiana in its commercial life, its Protestantism, its formal governmental structure, and its English background. The familiar note it strikes makes this study a particularly noteworthy contribution to the understanding of Negro culture in the New World.

HORACE MINER

University of Michigan

Social Control. By JOSEPH S. ROUCEK, Editor, and Associates. New York: D. Van Nostrand Co., Inc., 1947. 584 pp. No price indicated.

This introductory text for a university course in social control fills a distinct need in the field. Social control has become one of the focal points of sociological and socio-psychological interest ever since Edward A. Ross published the first book on the subject. Discussing the complex of means by which the agents of social control obtain uniformity of behavior, Ross developed a theory of regulative institutions to resolve conflicts among individuals and groups, while W. G. Sumner reduced control to mores

and patterns of conformity supporting an established social order. F. E. Lumley reduced the problem of control to a question of psychological pressure and L. L. Bernard concentrated all his attention to the techniques, while J. Dowd and P. H. Landis, elaborating on Cooley's views, emphasized the role of values, symbols and ideals in building an orderly society and integrated personality. The textbook now presented summarizes previous theory and research, but enlarges only the field of techniques to include much of the actually predominant factors of control (press, radio, movies, propaganda, etc.).

The editor has relied for his material on a number of experts whose contributions he has integrated into a cooperative product. H. C. Brearley, E. W. Gregory, Ch. W. Coulter, V. J. Parenton, and T. Lynn Smith contributed to Part I, which deals with the psychological foundations of social control. The co-authors of the chapters in Part II, institutional controls—state, law, religion, family, and education—were F. A. Cavé, A. Good, J. F. Cuber, P. F. Valentine, C. C. North, and M. J. Webster. In the most important section, Part III, where the means and techniques of control are considered, the editor took sole responsibility for the chapter on ideologies while N. de Nood, E. T. Arnesen, R. F. Bellamy, H. D. Meyer, L. D. Zeleny, Ch. F. Marden, H. B. Kirshen, G. E. Hoover contributed the other chapters. Paul Walter is credited with aid in organizing the whole project.

In general the book is well-organized, although in Part III chapters on language, art, literature, and recreation are thrown together with discussion of violent and non-violent means of control, leadership, secret societies, economic and social planning, without any clear systematic arrangement. Some of these topics would be better placed in the last section, which is a catch-all discussion of those contemporary problems of social control involving charismatic leadership and the totalitarian ways of life.

As a whole, the book is very useful, being particularly rich in material. Nearly a third of it is devoted to the pressing problems of ideological warfare and public opinion control: propaganda, press, radio, and motion pictures. Also, numerous questions and suggestions for term papers are included, as well as good bibliographies at the end of each chapter.

The general perspective of the work on some fundamental issues needs further comment. The treatment of the whole problem of social order is all-embracing. It is indeed praiseworthy that

the role of symbols and ceremonials, of conceptual techniques and cultural influences, of education and morals is stressed. At the same time, however, I would sharply criticize the philosophical level inasmuch as I am not "a nominalist at heart" (Clyde W. Hart A.S.R., Oct. 1947). Social processes and institutions cannot be understood if "concepts originate empirically"; of course, in Roucek's defense it might be said that a new theory of abstraction must be formulated and a systematic body of principles and values elaborated to deal adequately with social consciousness and similar conceptual problems in sociology. It is considered scientific to label the premises of social order as survivals or residues; then of course, the role of religion cannot be conveniently explained. The mere recognition of religious and political creeds is not sufficient. In our entirely politicized world the foundations of control are ethical and the cultural ends must be thoroughly understood if the theory of social control is to be more than a mere summary of means and techniques. The understanding, influence, and operation depends upon the standards and roles of the main agents of social control: the priest, the official, the teacher, and the media of communication and popularization. This aspect of social control is beyond the horizon of behaviorism and conformism.

GOTTFRIED SALOMON DELATOUR
Columbia University

The Police and Minority Groups, A Manual prepared for use in the Chicago Park District Police Training School. By JOSEPH D. LOHMAN. Chicago: Chicago Park District, 1947. xiii, 133 pp. \$2.00.

At a time when changing techniques and strategies in the field of race relations range from federal to state to local levels in a profusion of agencies, movements, committees and groups of humanitarian enthusiasts, this book represents a sober attempt to combine the scientific findings of sociology, anthropology and social psychiatry with the practical measures of police training in such a form that it can be appropriated directly by the patrolman of a local police force. The book results from the collaboration of the supervisory police personnel of the Chicago Park District and a sociologist (Joseph D. Lohman) who is associate director for race relations of the Julius Rosenwald Fund and associated with the sociology department of the University of Chicago.

As a manual for police officers this slender

volume fulfills its purpose admirably by presenting its materials in simple, yet scientific form on six selected topics: worldwide and neighborhood aspects of intergroup relations; the background of minority tensions; facts about race; situations in which tensions arise; and the role of the police officer in dealing with tensions. These topics represent the subject matter of the major chapters, each of which summarizes a single conference with police officers.

Mindful of his audience, Dr. Lohman stresses as a dominant theme the importance of the patrolman who represents the impartiality of law in the community, and he appeals to this motive in a manner calculated to win confidence; only inferential statements here and there hint that these motives have been weak or non-existent in the past. In chapter two there is extensive use of visual aids in the form of thirteen Chicago base maps showing population density, economic levels of living, and race or nationality areas with special emphasis on the border line regions (and the parks in them) where friction is likely to occur.

The section on the role of the police officer in dealing with tension will be of great interest to students of race relations and to police personnel in urban communities. The portrayal of stages in the growth of mob excitement and the proper techniques for dispelling crowds at each stage is masterly, both in its use of the findings of social psychology and in summarizing such practical devices as sufficient *show of force*, the cordon, etc. Another valuable feature of this section is an excerpt from one of Gordon Allport's studies of rumor which sketches vivid examples of its actual operation. The rumor study is preceded by a brief account of rumors in the Detroit and other race clashes.

The manual closes with two appendices containing the Illinois statutes regarding race relations and the Chicago ordinances establishing a kind of local F.E.P.C. At the end is a short but useful bibliography of books, pamphlets and films in the field of minority problems.

In spite of the undoubted usefulness of the manual, it raises two important questions: (1) Why was this volume prepared for a relatively small subdivision of the Chicago Police Department rather than for the entire force? If the natural surmise is correct that cooperation was forthcoming in the Park District Police but not from other branches of the whole department, it poses a new task for the Mayor's Committee and other interested groups to extend

this inaugural program into the rest of the police department. (2) Since this manual is so geared to local problems and situations that it could not be used without major modifications in other cities, would it not be profitable for the American Council on Race Relations or a similar organization to press for the same collaboration with the International Association of Chiefs of Police and thus gain entree into many urban communities with an International Manual of similar import? *The Police and Minority Groups* points the way.

R. A. SCHERMERHORN

Rhode Island State College

① *Social Relations and Structures: A Study in Principles of Sociology*. By E. T. HILLER. New York: Harper and Brothers, 1947. xii, 692 pp. \$4.50.

Systematization is important in science but it is a Sisyphean task. There is still no generally accepted systematic sociology. Hiller's book is not likely to remedy this. However, it is commendable to attempt something which is almost sure to fail since such failures may contribute to eventual success.

It is easy to state the criteria for a systematic sociology. It is also easy to give reasons why an adequate job is impossible at present. A basic text should reveal the current systematics of a natural science. Most physical-biological textbooks do this. A dozen basic physiology texts are much more similar than a dozen sociology texts.

It is revealing to compare Hiller's 1933 text with the present volume. Both are works in principles of sociology. The major headings are as follows: (1933) I. Social Relations and Institutions; II. Society as Communication; III. Mutual Aid and Competitive Co-operation: Functional; IV. Spatial Structure; V. Conflict and Social Structure; VI. Social Change; VII. Social Disorganization; VIII. Person in Relation to Culture and Social Organization; IX. Needs and Methods of Social Control; (1947) I. General Characteristics of Our Society; II. The Cultural Basis of Society; III. Elements of Social Relations; IV. Valuation of the Person; V. Organization of Social Relations: Institutions, Groups, Societies; VI. The Structure of Society; Statutes; VII. The Unity and Continuity of Society.

The 1947 volume is either a revision of the earlier concept of systematic sociology or an addition to it. One suspects the latter since

seventeen of the forty chapters in the present volume deal with status which was scarcely mentioned in 1933. There is little statistical material in either book. In both, the method is chiefly illustration of the "principles" by examples which are mostly taken from common-sense, history, anthropology, and literature. About 400 names are cited (1947) of which about fifty are sociologists, about fifty are anthropologists, and the remainder mostly historians, philosophers, and literary people. In 1933, about 500 were cited, the volume was more fully documented, and more of the men mentioned were sociologists.

The major institutions are not used as an organizing principle in either book. They almost disappear into higher levels of abstraction, especially in 1947. The economic institution gets considerable attention (1947) but it is not treated systematically. It is discussed under such concepts as utility relations and economic statuses (five chapters) though "economic" is seldom used. The other institutions are treated under the status concepts insofar as they are treated at all. The distinctions between institutions, groups, associations, persons, and other social structures are not made very clear. One of the first requisites for a systematic sociology is the clear distinction and logical classification of all kinds of social structures.

Most of the discussion deals with the meaning of terms rather than the statement and implications of principles. It is doubtful that this attempt to clarify terms will greatly diminish the terminological confusion which is one of the serious obstacles to an adequate systematics. Some new terms seem especially dubious, e.g., nonassociational groups; nonsocial institutions; extrinsic and intrinsic valuation, accountability, and inviolability; negative social act; nonsocial cultural complexes, and so on. These terms all have normative connotations and illogical implications. "Key status" may be useful.

Chief defects. (Pure Bain, of course. Hiller would probably deny the validity of all these criticisms—and he might be right.) Failure to place sociology in logical relation to the other sciences, and especially the social sciences. Failure to relate social phenomena to physical and biological phenomena. Neglect of population. Use of social relations, a loosely defined idea, as a basic concept. (Symbolic interaction seems a better basic concept.) Failure to stress methods of research and especially the importance of mensuration and predictive generalization. In-

adequate treatment of stability (equilibrium) and change. Failure to treat social structures systematically, and especially the absence of a clear-cut analysis of the major institutions.

The style is simple and straight-forward. Hiller uses little of the figurative, literary language which mars so much sociological writing. The discussion questions are very good. Those who are interested in systematic social theory certainly should read the book.

READ BAIN

Reed College

Sociology, A Synopsis of Principles. By JOHN F. CUBER. New York: D. Appleton-Century Company, 1947. Pp. xiii + 590. \$3.50.

Of all types of book reviews appearing in sociological journals, I am certain that the reviews of an avowedly introductory textbook are the least fruitful. The numerous teachers of sociology are characteristically in quest of a new textbook a month after they have adopted the one which they are currently using, and in any case, they will sooner or later examine the book in question. On a matter so vital to their everyday comfort in the classroom, they are unlikely to accept the evaluations of any reviewer. No doubt many harried teachers of too-large classes of inadequately prepared Sophomores, of the perennially immature introductory students, and of students temporarily vagrant from their real vocational interests, will welcome Professor Cuber's attempt, as he writes in his Preface, to "meet the students where they are."

The author begins with two chapters on the field of sociology and "some attitudes and understandings." Sociology is defined as "scientific knowledge about human relationships." But unfortunately there is much in the remainder of the book which does not deal with the direct analysis of and classification of human relationships (for example, most of Part II and much of Part III). The simple exposition of the need for objectivity, the discussions on "common sense" knowledge, and on the use of the "case method" are clearly presented and well illustrated. Part II provides "Background Understandings from Cultural Anthropology," six chapters dealing with the meaning, variability, content, organization and development of culture. The definition of culture is ingeniously elaborated and illustrated in an entire chapter. Part III deals with "Background Understandings from Social Psychology," presenting chap-

ters on socialization, personality, wishes, the self, and individuality. Part IV is concerned with groups, race, population, ecology, urbanization and stratification. In Part V the author writes of institutions, the family, economic groups, government, education and religion. Part VI discusses organization, disorganization, conflict, social problems and social valuation. Almost all of Parts IV, V and VI are Sociology.

I was alienated by the tone of the Introduction in which the author states his intention of "meeting the students where they are," and casts aspersions on what he refers to as the "negative public relations angle" (a wretched phrase) of other texts. Then the author proceeds to write for the most part with admirable clarity and simplicity. However, in the attempt to meet the students, the author, somewhat sheepishly and rather coyly, intrudes popular phrases such as "off the beam," "an uncomfortable spot," "know all about," and some hundreds of others. Professor Cuber carefully places these in quotation marks to indicate an awareness of his verbal sins. I believe there is no gain in clarity and some cheapening of style.

Professor Cuber provides many excellently chosen illustrations. He has a flair for observation and selection of illustrations from everyday life and from the stream of popular communication, all of which make the book very interesting and teachable.

This book is not an integrated Principles of Sociology, nor does it exclusively and consistently analyze "scientific knowledge about human relationships." It might properly be labeled an "Introduction to Some Facts and Theories About Man, Society, and Kindred Matters." However, this is likewise true of all but one contemporary text. Some years ago Professor Cuber castigated the sociologists for their lack of clearly enumerated "principles." Now, I believe they might ask of him, "Where now are the principles?"

WILLIAM ALBIG

University of Illinois

- ◊ *Social Policies in the Making: A Dynamic View of Social Problems.* By PAUL H. LANDIS. Boston: D. C. Heath and Company, 1947. 533 pp. \$4.00.

This volume is intended for the college student in the lower division, and for a course in social problems or social science orientation. Its secondary title seems more appropriate for the contents than does the primary one, which is used also as the title for the final part of the

book. The twenty-six chapters are divided between the following parts: I. Dynamic Processes in American Society; II. Personal Adjustments to a complex Society; III. The Family-Social System of Our Transitional Society; IV. Problems of the Politico-Economic System; V. Social Policies in the Making. There are useful pictures (26), many charts (69), a considerable number of tables (41), brief descriptions of films for use with each chapter, review questions, and selected bibliographies.

Landis has produced a treatise which should appeal to the student of sociology whether he is reading on "Urbanization," "Secularization," "Personality in a Transitional Society," "The Small Family Pattern of the Companionship Family," "Providing Economic Security," "Improving Education as a Means of Adjustment," or any one of the many other chapters. This volume, unlike many in sociology, is neither "dry" nor provincial; instead the discussion on vital and timely topics will interest the student and provide the basis for lively and stimulating class discussions.

The author has summarized and interpreted a large number of sociological and social science studies which deal with contemporary phenomena and public thinking. Much of the analysis, like that on cities, family life, sex, and religion, is "realistic" and will serve to orient the college student to the problems and issues of the day. By contrast, however, there is really nothing on the dynamic and popular ideologies and movements of fascism, socialism, and communism, although reform capitalism is treated at length. The omission of these subjects means that the student will have no opportunity to escape his ignorance and confusion on these powerful and significant developments of the twentieth century. In the final chapter on "Improving Education as a Means of Adjustment," one finds various valid conclusions along with exaggerated claims for educational achievements. There is little mention, if any at all, on the lack of education about the great social ideologies and the widespread indifference, ignorance, and opportunism on the part of teachers and administrators.

Some scientific orientation to social problems, including the nonnormative (nonevaluative) viewpoint, should be presented in a volume of this nature to emphasize, among other things, the objective approach and to distinguish clearly between sociological science as one field and normative philosophy or ethics as another. This discussion also ought to make a good presenta-

tion of several theoretical views of social problems, not omitting the means-ends (functional) scheme of thought. Landis does not provide orientation of this type but certainly the undergraduate student needs background material of this kind in the current sociological era of the 1940's.

One finds many judgmental or evaluative statements running through the book, as on pages 133-34, but many of these if not all of them, might be overlooked since it is evident that the author is writing as an educator as well as a scientific sociologist.

It should not be concluded from the foregoing criticism that this book is not a useful addition to our sociological literature. Instead, this reviewer believes that Professor Landis has written a creditable and useful book which any teacher in the problems field will want to examine before selecting the next text for his course.

VERNE WRIGHT

University of Pittsburgh

As *You Sow*. By WALTER GOLDSCHMIDT. New York: Harcourt, Brace and Company. 1947. xvi + 288 pp. \$4.00.

The author, who is an Assistant Professor of Anthropology and Sociology at the University of California at Los Angeles, attempts to do three things in this book. In the first place, he presents the results of a study which he and his wife have made of commercialized farming in California. Then he argues that the urbanization of rural society characteristic of California will fairly soon become general throughout the nation. Finally, he advocates the formulation of a national farm policy based on the recognition of this growing urbanization of rural society.

The first eight of the volume's ten chapters are given over to the California study. The reviewer's reaction to this major part of the book is entirely favorable. Three towns with their surrounding farms are described in considerable detail. The towns are similar except for the average size of the surrounding farms. In Wasco, the community which was studied first and most completely, the farm units averaged 140 acres in extent. Dinuba has smaller farms, the average being 57 acres. Arvin has larger, about 500 acres on the average.

It is convincingly indicated that in spite of great variation in the size of farms, there is a general pattern of urbanized agriculture in California. There is large investment, intensive pro-

duction, complete commercialism, and the existence of impersonal relationships between employers and employees. There is a two-class social system. Upon the one hand, there are the members of what the author calls the *nuclear* group, the "functioning members" of the community. Then there are the *outsiders*, who are "not accepted into community life" and are "not considered in community affairs," however long they may have lived and worked in the community area.

The *outsiders* have been a changing group ethnically. First, there were the Indians who were impressed into service on the Spanish ranches. There followed the Chinese, the Japanese and Hindus, the Mexicans, the Filipinos, and, more recently, the destitute white migrants from Oklahoma and neighboring states. These last are establishing themselves as permanent but inferior residents of California's farming areas. Demand for their labor is uncertain and irregular, and their pay is low. They suffer in all sorts of ways from the lack of those benefits that laborers in the cities have increasingly been able to achieve for themselves. They are second-class citizens of a state and nation which call themselves democratic, with the implication that equality of opportunity is maintained for all of their people.

The author is less convincing when he argues that "California stands merely at the vanguard of what is the major trend in American agriculture." Had he studied as carefully, from within, the nature of the family-farm system as he studied the farms of California he would quite likely have been impressed by the "staying qualities" of the traditional American farm as regards its more fundamental characteristics. Even as it is, he does see reasons for the situation in California to be different from those in most of the rest of the nation. At the outset, there was the heritage from Spain which was "a heritage of large landholdings," which gave the "background for the present agricultural pattern." He mentions, also, the nature of California's climate and terrain as being such as to make industrialized farming profitable. He shows, too, that he understands that "the tradition of the small independent farmer has spread throughout most of the nation." And he says that "there are well over two million commercial farmers who hire no labor" or only supplemental labor to that of themselves and families. A close look at those two million farm families would quite surely reveal forces at work that are destined to assure that much the same

situation will prevail in the oncoming generation.

The author in presenting his view that the complete urbanization of American agriculture is inevitable remarks, "It would be difficult to conceive that there could be any turning away from mechanization of farming; and it would be equally difficult to visualize such mechanization without growing industrialization and the urbanizing influences that follow." If, however, the essence of the traditional family-farm system is to be found in the working together of the members of the family as a unit, with the hiring of only supplemental labor, then increased mechanization may do nothing to affect that essence. Mechanization may make for increased average size of farms without doing away with the traditional family-centered nature of the enterprise, which in general distinguishes it from urban industry.

One must agree with the author that a truly democratic society would give its farm laborers a better life than America's are enabled to achieve, and this whether or not they exist in great concentrated groups as on the industrialized farms of California. The advocacy of improved national policy, however, is likely to accomplish little. Our society is of such a nature that in general economic rewards are measured out in proportion to the economic power that the various groups are enabled to bring to bear in pursuit of those rewards. Farm laborers have little economic power, and are forced to suffer in consequence. It would take a fundamental change in our values away from individualism toward cooperation to bring into being the sort of agricultural life that the author and many others would like to see develop in America.

ROY HINMAN HOLMES

University of Michigan

Modern Economic Thought: the American Contribution. BY GRUCHY, ALLAN G. New York: Prentice-Hall, Inc. 1947. 670 pp. \$5.65.

Psychologists and sociologists have long criticized classical economists on the ground that their mode of analysis was highly artificial and their view of human nature extremely limited. We have, of course, been aware that there were economic writers who adopted a more realistic orientation toward the facts of human behavior, but it has been easy to overlook their importance and the magnitude of their contributions. Professor Gruchy has therefore accomplished a valuable service by preparing this volume on heterodox economic thought.

As classical economics was based on an atomistic approach involving free competition under stable and unchanging conditions, it is not surprising that the opposition to orthodoxy centered around a *holistic* philosophy. Instead of assuming that individuals competed in something of the mechanical manner of molecules bumping against each other in a gas chamber, these economists accepted the fact that the whole social order, especially the existing economic institutions, influenced the amount and manner of competing. The change in viewpoint is thus parallel to that from Newtonian physics to relativity theory, and from structuralism and behaviorism to Gestalt and field theory in psychology.

Gruchy develops his material by analyzing the contributions of six well-known economists: Thorstein Veblen, John R. Commons, Wesley C. Mitchell, John M. Clark, Rexford G. Tugwell, and Gardiner C. Means. In each case considerable attention is given to the intellectual antecedents, personal development and outside influences which may reasonably be presumed to have affected the individual's economic theories. This is a pleasant relief from those evaluative summaries which seem to assume that a man's views are developed within himself by a process of pure reason. It suggests that Professor Gruchy also adopts a holistic approach to his problems.

The common feature which runs through all six of these economic theories is an acceptance of the economic system as a "going concern"—an emphasis upon process and change, as opposed to static and mechanical analysis. The phenomena so familiarly treated under the rubrics of competition, supply and demand, marginal utility, and so forth, do not exist as pure abstractions. If they occur at all, it is always within a framework of existing economic institutions and customs. Thus the parts are determined by the whole of society, rather than—as in classical theory—the parts giving us the essential character of the whole. "From the viewpoint of the holistic school," writes Gruchy, "*economics is the study of the structure and functioning of the evolving field of human relations which is concerned with the provision of material goods and services for the satisfaction of human wants.*" (p. 550).

This definition suggests that the holistic group is also characterized by a difference in psychological orientation from that of the classical school. Gruchy gives ample evidence to verify this belief. As he says, the holistic theory

"takes the individual to be a participating member of the economy as a going concern or system. This going system conditions the behavior of the individual by setting up certain collective standards or norms of conduct. The individual is therefore treated as a social being whose behavior is largely collective and habitual." (p. 19). This psychological view is necessarily more complex than that of the classical economist, who relies upon the hedonistic calculus to explain human choice, and postulates purely acquisitive, competitive motivation as his driving force. The holistic economists are keenly aware that man has cooperative as well as competitive impulses; that his decisions and actions are often determined by institutional and habitual patterns rather than by self-interest; and that irrational expectancies often affect his economic activities. Such a theory makes impossible the neat graphs and formulae of the classical school; but it is immeasurably closer to human reality.

Since the holistic economists wish to treat the economic system as a going concern, they cannot deal in magnificent abstractions. They must identify the whole system of which their economic data form a part, and they have therefore necessarily concentrated on specific economic phenomena—usually the situation in the United States in the early twentieth century. While this has prevented them from evolving universal formulae, it has given their work a relevance to immediate conditions which no doubt accounts for their influence on national policies under the "New Deal."

After Gruchy has developed a rather careful analysis of the theoretical writings of each of these six individuals, he tries to draw some important generalizations about holistic economics in a final chapter. One is tempted to suggest that this should have been longer and the earlier material abbreviated, to reduce repetition; but, after all, Gruchy is presumably expounding the ideas of others rather than his own. Thus he indicates what he considers to be the basic assumptions of the holistic economists, and pays special attention to their price and distribution theory. This is followed by a final section which suggests that the holistic economists will have a significant influence upon the future development of our economic order.

The author's style is on the whole clear and readable. His organization suffers from repetitiveness, some of it necessary; much, I believe, could have been avoided. Nevertheless, his book merits and will probably receive wide

attention. Specifically it should indicate to psychologists and sociologists that not all economists can be ignored on the ground of their hopelessly inaccurate conception of human behavior. One might suggest that psychologists and sociologists should now reciprocate by devoting more attention to the impact of economic conditions upon their own subject-matter.

I was interested to note in passing that virtually all holistic economists seem to be reformers. Momentary reflection on this point led me to the following proposed solution: the classical economist deals with mechanical material in a static framework, so he must become a conservative. The holistic economist recognizes that change is an intrinsic feature of all economic systems; his material is a dynamic process. He is thus stimulated to plan for a modification of the system in a direction which seems to him more desirable.

ROSS STAGNER

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Marriage and the Family. By MEYER NIMKOFF.
Boston: Houghton Mifflin, 1947. xx + 767
pp. \$5.00.

An important contribution has been made by Professor Nimkoff to the extensive list of text books concerned with marriage and the family. He recognizes the dual nature of objectives in the field, namely to present the sociology of the family and to provide practical guidance for persons interested in marriage and parenthood. He believes that both needs can be satisfied by a single book. He comes near to proving his point by his massive synthesis of up-to-date material in the family area. The book is written for students and not for academic sociologists and hence does not attempt to shape any original theoretical structure or to introduce completely novel formulations. His is the usual stock in trade of the sociologist; technological and economic causation, culture lag, family functions, and the more common sense psychoanalytic concepts. Among the more original contributions would be a fairly systematic attempt to relate family forms to economic structure and a conception of the family as a relatively static component of an institutional matrix.

The scope and content of the book are really impressive. It is a generous book. Professor Nimkoff has read widely and thoroughly and has spared no effort in making the returns from his vast labors conveniently available to students. The book is divided into four parts. There is an introduction which presents the background of

the family with stress on material culture. Part II describes in detail the modern American family in its cultural setting with stress on family variability in the sub-cultures of the United States. Especially illuminating is the discussion of ethnic variations. Part III deals with marriage and personality. This is the marriage course material presenting richly the sounder evidence concerning personality formation, mate selection, marital prediction and family adjustments. The final section is concerned with the family and social change. The student will find here a wealth of pertinent information including suggestions as to "what to do about it." The ethical assumption seems to be approval of a national pursuit of collective happiness guided by science within our own general cultural framework.

The book provides every possible aid to the student. There are pictures, pictorial statistics, diagrams, maps, tabular presentations of laws by states, adjustment and prediction scales, sensible discussion questions, report topics and annotated bibliographies. If a student wants to be guided by facts he will find information in abundance and competent guidance in drawing his own conclusions and applications.

A book so good can withstand comment on its apparent weaknesses. To the reviewer the following points seem pertinent.

1. The book is a trifle repetitious in organization.
2. There is excessive detail concerning socioeconomic aspects of the American family. (There must have been a temptation to draw on Ogburn and Nimkoff.)
3. There is lack of vigor in grappling with definition and typology.
4. An occasional lapse of documentation es-

pecially on critical points strikes the reader unfavorably.

5. Occasional reasoning from partial evidence concerning factors in marital adjustment is to be found. (For instance, switching from Burgess-Cottrell to Terman and ignoring contradictory evidence.)

6. Uncertainty as to evaluation of population trends leaves the reader confused.

7. An occasional careless statement is found which might undermine the confidence of fiery youth in the realistic wisdom of the older generation. (For example, p. 719, "Some women experience two or more orgasms in an act of coitus whereas a man can have only one." The worried veteran could be assured that the passage probably refers to particular males.)

8. The quality of analysis is uneven. Various examples might be given of the last point. It may be doubted that stopping reproduction with male offspring would change the sex ratio any more than stopping throws when a penny came down heads (p. 567). High incidence of remarriage among divorced persons may be due to picking a second mate thus causing divorce (p. 642). Some students may have expressed respectable rather than real opinions on the Reuben Hall study (p. 732).

Minor flaws only accentuate the virtues of Nimkoff's book. The clarity of writing is enviable. In no other single book could a student find so much useful and reliable material concerning marriage and the family. Many thousands of students will read this fine text and become better informed. Some will also become wiser, some happier. In any event Professor Nimkoff has labored greatly and achieved greatly.

CLIFFORD KIRKPATRICK

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BOOK NOTES

City, Region and Regionalism, A Geographic Contribution to Human Ecology. By ROBERT E. DICKINSON. London: Kegan Paul, Trench, Trubner & Co., 1947. xv, 327 pp. 21s.

The reader will search this book in vain for a contribution to human ecology, geographic or otherwise. It consists rather of a summarization of published research on urban and metropolitan spatial patterns in the United States, England, and Europe. No new research is reported. So inclusive is the author's review of research studies, however, that the volume will find considerable use as a handbook.

In the final five chapters the author wades into the verbal morass of "regionalism," and like so many others before him he fails to drain the concept of its many ambiguities. "Regionalism" remains a curious concoction of sentiment and fact, of indefinite form and vague content, which somehow or other is supposed to recommend itself to the planner.

The Economic Almanac for 1948: A Handbook of Useful Facts about Business, Labor and Government in the United States and Other Areas. By The National Industrial Conference Board. New York, 1948. ix-472.

Useful compendium of statistical information on population, public and private debt, banking and finance, prices, American enterprise in general, manufacturing and various other industries, resources, the labor force, public finance, individual savings and national wealth, consumption and the standard of living, the national income, U. S. foreign trade, the international financial position of the U. S., international economic statistics, and a list of 87 alphabetical agencies of the federal government. Puts U.S.S.R. population, 1960, at 228 millions; population of U.S.A., 1960, at 148 to 154 millions. Indexes unions in three places; strikes, four; labor movement, zero.

Problemes Humains Du Mechinisme Industriel.

By GEORGES FRIEDMAN. Abbeville, France: Librairie Gallimard, 1946. 387 pp. (paper) Price not indicated.

Interesting proof of competency of a French author to summarize and criticize the work of Taylor, Mayo, and other students of industrial problems in the English-speaking world. Contributes nothing essentially new, Friedman's

thesis being that machine industry has made industrial relations extremely complex and difficult and that scientific studies such as those of Mayo and of various social psychologists fail to face the larger issues involved, namely, conflicts of power and differences in values, among the owners of industry, the workers, and the general public.

Some Notes on the Psychology of Pierre Janet.

By ELTON MAYO. Cambridge: Harvard University Press, 1948. vii + 132. \$2.50.

A curious book by any standards of social psychologists, psychiatrists, or industrial sociologists. The man mainly responsible for applying experimental methods to the study of human relations in industry recommends the ideas of one of the pioneers of mental therapy, Janet's concepts of hysteria and obsession, to explain certain aspects of individual adjustment to industrial situations, without more than passing reference to Freud or to any later developments in the field. A 15-page appendix on "frightened people," Mayo's own analysis of three levels of social insecurity, is the most suggestive part of the book.

The Labor Leader: An Exploratory Study. By

ELI GINZBERG assisted by JOSEPH CARWELL. New York: Macmillan, 1948. xiv + 101. \$3.00.

A superficial analysis of the tenure of office and of the financial resources controlled by the 627 executive committee members of ten American unions between 1900 and 1940. The ten include among others the United Mine Workers of America, the Brotherhood of Railroad Trainmen, the United Brotherhood of Carpenters and Joiners of America and the International Typographical Union. Results are not to be compared with Slichter's data in *The Challenge of Industrial Relations*. Dr. Ginzberg concludes, after several sketchy chapters on the nature of leadership, that American business unionism has shot its bolt and must adapt itself to political action. The one contribution in the book, based on the participant observations of the junior author, is a chapter of 87 pages describing the organization and early struggles of the Parkinstown Local of the Amalgamated Pottery and Porcelain Workers' Union in Parkinstown, Pa., in 1936-37.

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